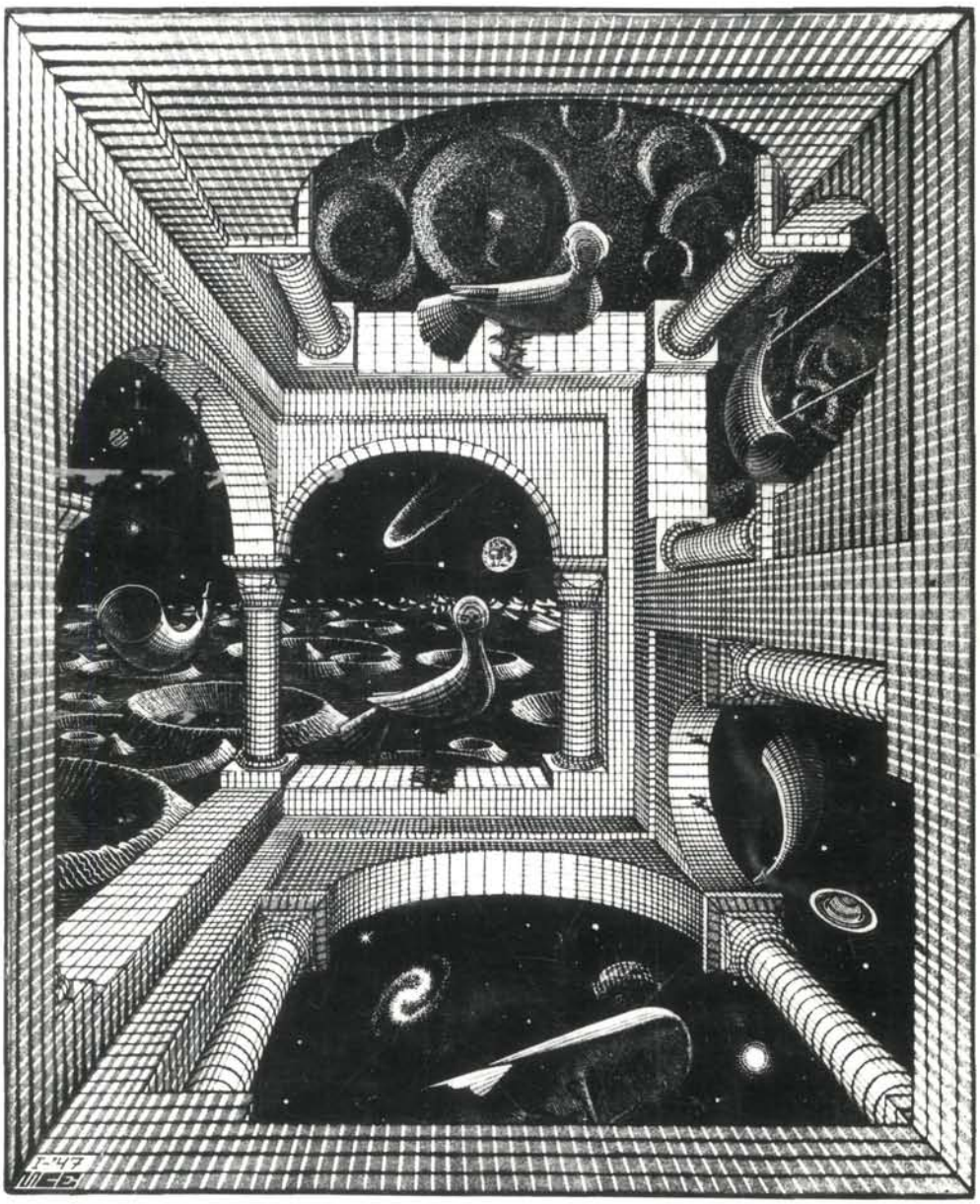
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In This Issue

In 1985, we published a two-part article by **Thomas L. Haskell**, "Capitalism and the Origins of the Humanitarian Sensibility." The article has evoked challenges from two scholars, **David Brion Davis** and **John Ashworth**. The forum in this issue features the critical remarks of these two historians followed by a reply from Haskell.

David Brion Davis, a target of Haskell's critique in the earlier article, seeks to rectify misinterpretations of his thesis on the hegemonic character of the antislavery thought. Davis contends that, in arguing that the antislavery movement mirrored the needs and tensions of a society increasingly absorbed with problems of labor discipline, he was not relying on such needs and tensions alone to explain the emergence of abolitionist thought and the direction it took. Instead, in response to both proslavery and radical indictments of the wage-labor system, most early abolitionists accentuated the moral contrast between what they conceived of as two worlds: free and slave. Abolitionists, who hoped that black slaves could be transformed into voluntary wage workers, acted on the assumption that the British system of labor had achieved a reasonable balance between freedom and order and could serve as a norm against which harsher regimes should be measured.

John Ashworth provides another contribution to the debate with a critique of the reasoning of both Davis and Haskell. Ashworth argues against "self-deception" on the part of the abolitionists, as Davis has described it, and revives the concept of "false consciousness," defining it as an outlook imposed by one's circumstances that can move a person to act on class interest without intending to further those interests either consciously or unconsciously. The bourgeoisie promoted an unfettered market and wage labor but understood that, for society to survive these assaults on the traditional foundations of order, competition had to be barred from some arenas. In the new conditions, the individual conscience and the family were understood as the guarantees of morality and disinterested love. Because slavery constrained conscience and constituted an outrage on the family, it was the obvious target of those who promoted the market and wage labor.

In his response, **Thomas L. Haskell** points out that Davis assigned a larger role to hegemony in his book *The Problem of Slavery in the Age of Revolution* than he now does. Haskell denies that Davis's new formulation has supplied an adequate warrant for believing that the abolitionists unconsciously intended the hegemonic consequences of antislavery agitation. He argues that the rise of humanitarian sensibility had less to do with class interest than with a shift in the network of conventions that define personal agency and govern the assignment of blame and responsibility to oneself and others. Defending the explanatory category of convention from the charges of determinism and amorality that Davis and Ashworth level at it, Haskell maintains that people unavoidably rely on tacitly understood conventions in deciding what they can and ought to do to help others in distress. The question of why the reformers attacked chattel slavery instead of opposing all forms of labor exploitation is, according to Haskell, badly chosen. Granting that the abolitionists were more concerned with the misery of slaves than that of free laborers, he attributes this attitude not to class-induced self-deception but to a realistic assessment of reform priorities coupled with a formalistic understanding of freedom that made free workers appear to be largely responsible for their own fate.

In a review essay of a dozen recently published works in intellectual history, **John E. Toews** examines William Bowsma's projection of a merging during the 1980s of the subdiscipline of intellectual history into an interdisciplinary history of meaning with a strong linguistic slant. Although Toews discerns the expansion and maturation of this trend, he contends that it has not resolved traditional problems and controversies in intellectual history so much as translated them into a new vocabulary. Toews views with uneasiness the tendency to reject the social history of ideas and accept instead the reduction of experience to the meanings that shape it. He argues for a dialectical approach that affirms the irreducibility of both poles of meaningful experience, connects the concerns of the younger generation of historians more clearly to the achievements of the older, and revives the effort to conceptualize the mediating links between life and text.

Richard Harvey Brown reviews two new works on the logic of method in the historical sciences. In a detailed discussion of books by C. Beham McCullagh and by Ernest Gellner, Brown treats the position of each author as a counterpoint to that of the other and the two together as stages in a larger dialectic between positivist materialism and romantic idealism. Brown's review vindicates narrative discourse but does so from a decidedly post-positivist approach to the epistemology of historical knowledge, one that is built on linguistic self-consciousness.

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Richard Harvey Brown's research interests are in political economy and cultural psychology and in the interaction of these two fields in developing societies and advanced capitalist states from a perspective of comparative history. He is also a theorist of the discourses of the social sciences in general, as represented by his major works, *A Poetic for Sociology: Toward a Logic of Discovery for the Human Sciences* (1977), *Structure, Consciousness, and History* (1978), with Stanford Lyman and others, *Society as Text: Essays on Reason, Rhetoric, and Reality* (1987), and *Social Science as Public Discourse: On the Invention, Legitimation, and Uses of Social Theory* (Chicago, forthcoming). Brown is an associate professor in sociology at the University of Maryland, College Park, and president of the Washington Institute for Social Research. He earned his degrees at Columbia University and the University of California (Berkeley and San Diego). He is now writing a book with Remi Clignet, *At the Edge of History: America in the Post-Modern Era.*

David Brion Davis is Sterling Professor of History at Yale University. His main fields of interest are cultural and intellectual history, the history of reform, slavery and emancipation, and race relations. Davis did his graduate work in the History of American Civilization program at Harvard University. His most important publications are *Homicide in American Fiction, 1798–1860: A Study in Social Values* (1957), *The Problem of Slavery in Western Culture* (1966), *The Problem of Slavery in the Age of Revolution, 1770–1823* (1975), *Slavery and Human Progress* (1984), and *From Homicide to Slavery: Studies in American Culture* (1986). He is now writing the final volume of his trilogy, which will be entitled *The Problem of Slavery in the Age of Emancipation.*

Thomas L. Haskell, professor of history at Rice University, is at work on a study of changing perceptions of causation and moral responsibility in Anglo-American culture since 1750. The article in this issue reflects both that work and his long-standing interest in problems of historical explanation, especially at the boundary between social and intellectual history. He is the author of *The Emergence of Professional Social Science: The American Social Science Association and the Nineteenth-Century Crisis of Authority* (1977) and the editor of *The Authority of Experts: Studies in History and Theory* (1985).

John E. Toews is an associate professor of history and chair of the Program in the Comparative History of Ideas at the University of Washington. He completed his graduate studies in European Intellectual History under the direction of H. Stuart Hughes at Harvard University in 1973. Before moving to the University of Washington in 1979, he taught for six years at Columbia University. His publications include *Hegelianism: The Path toward Dialectical Humanism, 1805–1841* (1981) and articles on the history of the psychoanalytic movement. Awarded a MacArthur Prize fellowship in 1984, he is currently working on a multivolume study of attempts to reconstruct the foundations of ethical and cultural values on historical grounds among speakers for competing intellectual and political movements in Germany during the 1840s.

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AHR Forum

Reflections on Abolitionism and Ideological Hegemony

DAVID BRION DAVIS

ATTENTION IS TURNING ONCE AGAIN TO THE ALMOST SIMULTANEOUS APPEARANCE of industrial capitalism and antislavery sentiment in Great Britain. Since the publication of Eric Williams's *Capitalism and Slavery* more than a generation ago, the relation between these two broad forces has provoked considerable debate. Historians have discredited Williams's argument that Britain's antislavery measures were economically determined acts of national self-interest, cynically disguised as humanitarian triumphs.[1] But it has been difficult to find a middle ground that rejects Williams's cynical reductionism while also taking account of the realities of class power. In 1975, in *The Problem of Slavery in the Age of Revolution, 1770–1823*, I suggested that British abolitionism served conflicting ideological functions but that it helped reinforce, in this initial period, the hegemony of capitalist values. This view has recently evoked fruitful criticism, especially a two-part theoretical essay by Thomas L. Haskell in *The American Historical Review*.[2]

[1] See especially Roger Anstey, *The Atlantic Slave Trade and British Abolition, 1760–1810* (London, 1975); Stanley L. Engerman, "The Slave Trade and British Capital Formation in the Eighteenth Century: A Comment on the Williams Thesis," *Business History Review*, 46 (Winter 1972): 430–43; Howard Temperley, "Capitalism, Slavery, and Ideology," *Past and Present*, 75 (1977): 94–118; Seymour Drescher, *Econocide: British Slavery in the Era of Abolition* (Pittsburgh, Pa., 1977); Seymour Drescher, "Cart Whip and Billy Roller: Antislavery and Reform Symbolism in Industrializing Britain," *Journal of Social History*, 15 (September 1981): 3–24; Seymour Drescher, *Capitalism and Antislavery: British Mobilization in Comparative Perspective* (New York, 1987); *British Capitalism and Caribbean Slavery*, Barbara Solow and Stanley L. Engerman, eds. (New York, 1987); and David Eltis, *Economic Growth and the Ending of the Transatlantic Slave Trade* (New York, 1987).

[2] Thomas L. Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 1," *AHR*, 90 (April 1985): 339–61; Part 2, *AHR*, 90 (June 1985): 457–566. See also the relevant article by T. J. Jackson Lears, "The Concept of Cultural Hegemony: Problems and Possibilities," in *AHR*, 90 (June 1985): 567–93. Although my response to Haskell meets some of the criticisms advanced by Betty Fladeland and Seymour Drescher, I have tried to deal with their substantive arguments, many of which are compatible with my own views, in a different version of this article, "Capitalism, Abolitionism, and Hegemony," in Solow and Engerman, *British Capitalism and Caribbean Slavery*, 209–27. See Betty Fladeland, *Abolitionists and Working-Class Problems in the Age of Industrialization* (Baton Rouge, La., 1984); Drescher, "Cart Whip and Billy Roller," 3–24; and Drescher, *Capitalism and Antislavery*.

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Because I bear some responsibility for the misinterpretations that have been given to my "thesis," I would like to take this opportunity to restate and clarify my argument and to assess some of the criticisms.

I should first point out that my hegemonic argument fills only a few pages in a 570-page volume and that it applies only to British history in a limited period from the 1790s to 1823, with some brief speculations reaching ahead to the 1830s. I did not extend the concept of hegemony to America or France, where abolition movements emerged in wholly different contexts. Certainly, I advanced no general theory of abolitionism *per se* as an instrument of hegemonic control (although I would claim it was always related to the need to legitimate free wage labor). I have never meant to suggest that abolitionism can best be understood as a device for deflecting white working-class discontent or that it was not part of the wider egalitarian and liberalizing movement I described in *The Problem of Slavery in Western Culture* (1966).

It is important to distinguish the *origins* of antislavery sentiment, a subject I discussed at length in the first volume, from the *conditions* that favored the acceptance of antislavery ideology among various governing elites. This is a distinction that Haskell continually blurs. In all my work, I have taken pains to emphasize the importance of religious sources of antislavery thought and the religious transformations that made slave emancipation a symbolic test of the efficacy of Christian faith. In *The Problem of Slavery in the Age of Revolution*, I did not say, and here Haskell misquoted me, that the "origin" of the new humanitarian sensibility lay in "the ideological needs of various groups and classes."[3] I did maintain that "the continuing evolution" of antislavery opinion "reflected the ideological needs of various groups and classes."[4] I had in mind the ideological needs generated by the French Revolution and the early Industrial Revolution, by war, nationalism, and religious revivalism. At issue are the uses made of antislavery doctrine and rhetoric as the movement pulled away from the Painite radicals of the early 1790s, won legitimacy from government ministries in 1806–07, was appropriated by the aristocratic African Institution, and was then reshaped by wealthy merchant philanthropists.

In *The Problem of Slavery in the Age of Revolution*, I dealt with Britain in the period from 1793 to 1823, decades of reactionary politics and domestic repression that should not be confused with the era of social ferment and reform that accompanied West Indian slave emancipation and the abolition of apprenticeship. The crucial question, therefore, was not why groups of enlightened British, French, and Americans attacked slavery from the 1760s to the 1780s but why this single reform cause, which attracted significant radical support in the early 1790s and which some conservatives denounced as a Jacobin front, won growing acceptance in the early nineteenth century from British political and social elites otherwise obsessed with the fear that social reform would open the gates to revolution.

[3] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 1," 344.

[4] David Brion Davis, *The Problem of Slavery in the Age of Revolution, 1770–1823* (Ithaca, N.Y., 1975), 42.

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During the long period from the late 1790s to 1823, the British public showed little interest in the slavery issue except at the end of the Napoleonic wars. In 1814, an eruption of petitions expressed outrage at the prospect that the government would allow France to resume the Atlantic slave trade, which Britain had earlier renounced on moral grounds. This brief popular outburst drew on nationalistic pride and was orchestrated by abolitionist leaders who were eager to demonstrate to the courts of Europe that "with a single voice" the English people demanded international suppression of the slave trade. The cause served the purposes of Wellington and Castlereagh, who actively cooperated with the abolitionists. But the crucial antislavery measures from 1800 to 1823 were not the result of public pressure. Leaders of government and a few influential abolitionists were responsible for the decisions to curtail and then stop the flow of African slaves to Guiana and other foreign colonies conquered by Britain and later to prohibit the British slave trade to all foreign nations and colonies. The successful abolition bill of 1807 originated in the House of Lords; the prevailing public apathy and ignorance of the question prompted William Wilberforce to publish and widely circulate *A Letter on the Abolition of the Slave Trade: Addressed to the Freeholders and Other Inhabitants of Yorkshire*. The subsequent campaign to establish a central registry of all colonial slaves aroused little public interest, although it was seen as an essential preparatory step toward amelioration and gradual emancipation. Even in 1823 and 1824, when an organized emancipation movement got underway, the abolitionists who solicited petitions and organized auxiliary societies were surprised by the general public ignorance concerning West Indian slavery. Yet the governing elites, while still divided on the issue, had become increasingly committed to colonial labor reform. By 1833, even the Tory opposition in the House of Commons failed to rally behind the West Indian interests.[5] In pursuing the question of why colonial slavery seemed so repugnant to such groups, I should have made it clearer that by "ideology" I did not mean a fixed set of ideas and doctrines used to promote concrete class interests. When referring to an ideology as a "mode of consciousness," I was thinking of a perceptual lens, a way of viewing social reality that helps to define as well as legitimate class, gender, or other collective interests. Keeping this definition in mind, it is important to draw a distinction between the motives of individual reformers and the ideological context that gave hegemonic meaning to their rhetoric and influence.

When, in 1786, Thomas Clarkson published his prizewinning Cambridge University essay on the horrors of the African slave trade, he clearly had no intention of condoning British child labor in factories and mines. But Clarkson's proslavery opponents, such as Gilbert Francklyn and Jesse Foot, immediately contrasted the alleged comfort and security of West Indian slaves with the oppression of English workers and the plight of English children exposed to the "pestilential vapour" of factories. Francklyn pointedly asked why the universities

[5] Eltis, *Economic Growth*, chap. 2. Eltis argued that abolishing the slave trade and emancipating colonial slaves were contrary to Britain's economic interests but were the result of a free-labor and free-market ideology that led to the New Poor Law of 1834 and to concerted efforts to increase the productivity of labor.

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did not offer prizes "for the best dissertation on the evil effects which the manufactures of Birmingham, Manchester, and other great Manufacturing towns, produce on the health and the lives of the poor people employed therein?" He proceeded to show how Clarkson's rhetorical techniques could be applied to the specific consequences of the early Industrial Revolution. Francklyn was probably not surprised when Lancashire manufacturers took the lead in 1787 in initiating the great petition campaign to abolish the African slave trade.[6] Antiabolitionist counteroffensives such as these had appeared even earlier, and they became a dominant theme of British and, later, American proslavery writing. Similar points were made by representatives of radical labor who were in principle opposed to all forms of economic and political bondage. Given the venom of the debate, no abolitionist could plead ignorance of the charge that moral outrage was being directed against oppression overseas while similar or worse oppression was complacently tolerated at home. In 1818, for example, Francis Burdett asked why Wilberforce could be shocked by the enslavement of Africans and yet support in Parliament a seditious meetings bill and the suspension of *habeas corpus*, measures that allowed the English to be seized and treated like African slaves.

In theory, abolitionists faced by these challenges could condemn all forms of social oppression and simply give priority to the slave trade or chattel slavery as the most flagrant and remediable crimes against humanity. Such a course would entail a disavowal of the claims of proslavery writers and at least a private expression of regret over the unintended consequences of extolling free wage labor. As a second alternative, exemplified by some of the radical Garrisonians and labor reformers, the abolitionists could assert that both distant and nearby evils arose from a common cause. As a third choice, they could deny any comparability between black slaves who were subjected to constant physical coercion and English workers who faced merely the threat of starvation, which was termed a "liberal motive" and a "rational predicament" by the reformer who drafted the slave emancipation act of 1833.[7]

In response to both proslavery and radical indictments of the wage-labor system, most abolitionists accentuated the moral contrast between what they conceived of as the free and slave worlds. Their greatest hope, after all, was to end the involuntary shipment of Africans to the New World and to transform black slaves into cheerful, obedient, and grateful laborers whose wants could be satisfied only by working voluntarily for wages. This hope rested on the assumption that the British system of labor had achieved a reasonable balance between freedom and order and could serve as a norm against which harsher regimes should be measured. I am not suggesting that early abolitionists were mostly conservatives who accepted the status quo and opposed domestic reforms, although some of them fit this description. But the sharp contrast they drew between British and

[6] Cited in Davis, *Problem of Slavery in the Age of Revolution*, 462–63. Because I am responding to misreadings of this book, I will take most of my examples from it. Seymour Drescher has recently drawn attention to the crucial role of Lancashire industrialists in initiating the antislave trade campaign (*Capitalism and Antislavery*, 67–88).

[7] David Brion Davis, *Slavery and Human Progress* (New York, 1984), 218.

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colonial society had ideological meaning, especially at a time when there was a growing need to valorize wage labor as a universal norm, when the Industrial Revolution was introducing new forms of exploitation and suffering, and when it was by no means clear that the British working class was less victimized than were West Indian slaves.

For example, early in 1807, at a depressing stage in Britain's war against Napoleon, James Stephen the elder, who was the master strategist for the abolitionists, singled out British depravity in Africa and the West Indies as the cause for God's vengeance. Stephen specifically excluded domestic sins and proceeded to marvel over the "social happiness [that] has been showered upon us with singular profusion." "In no other part of the globe, are the poor and helpless so well protected by the laws, or so humanely used by their superiors . . . If it be as the protector of the poor and destitute, that God has entered into judgment with us, we must, I repeat, look to Africa, and to the West Indies, for the causes of his wrath."[8] Stephen was a deeply religious man who was genuinely concerned with collective guilt and retribution. We can be almost certain that he did not consciously intend to use his abhorrence of slavery and the slave trade, which he had observed firsthand in the West Indies, as a means of diverting attention from domestic suffering. Although as a boy he and his mother had lived in debtors' prison, he honestly believed, at least after marrying into the Wilberforce family and allying himself with paternalistic Tories, that Britain's treatment of its poor could not be a cause for divine displeasure. Later in 1807, Stephen played an important role in securing the abolition of the British slave trade, in a law hailed by political leaders as the most altruistic act since Christ's crucifixion and as proof that Britain waged war for human brotherhood. National pride is especially dangerous and deceptive, as Reinhold Niebuhr reminds us, when it is based on the highest achievements of human history.[9]

Haskell has argued that it is difficult if not impossible to reconstruct a historical actor's unconscious intention or to distinguish between consequences that are unconsciously intended and consequences that are unintended and even random. This difficulty, in his view, invalidates any historical explanation based on the concept of self-deception. I would reply that the concept of self-deception is so central to any quest for self-understanding or social analysis that it cannot be discredited by a kind of positivistic behaviorism. In everyday life, people continually recognize self-deception without resorting to depth psychology or finding the source of unconscious intention. To cite one obvious example, advertising agencies know they can rely on self-deception when they link cigarette smoking or the consumption of "lite" alcoholic drinks with health, slim figures, and vitality. Because the actual effects of smoking cigarettes and consuming alcoholic beverages are well known to the public, the success of the advertisements cannot be attributed simply to ignorance or gullibility.

[8] Quoted in Davis, *Problem of Slavery in the Age of Revolution*, 366–67. Wilberforce also warned in 1807 that Britain's afflictions might be a prelude to much worse divine punishment if the nation persisted in the criminal slave trade (*Letter on the Abolition of the Slave Trade* [London, 1807], 4–6).

[9] Richard Wightman Fox, *Reinhold Niebuhr: A Biography* (New York, 1985), 181.

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A subtle complicity often exists between self-deception and externally imposed deception that produces what some historians would call "false consciousness." That term carries unfortunate connotations that lead to unnecessary controversy. But, as historians, we do need to recognize the reality of deceptive or biased consciousness—of collective rationalizations, sometimes consciously crafted, that serve identifiable interests and help convince individuals of their own innocence and virtue. Biased consciousness, which Haskell tried to subsume under the morally neutral term "convention," also shields individuals from any sense of complicity in social injustice and oppression. The abolitionists dramatized the personal guilt entailed by one form of biased and deceptive consciousness. Modern history, from the Holocaust and Vietnam War to the peril of Star Wars and nuclear annihilation, teems with examples of individual and collective self-deception. Although biased consciousness can result from ignorance as well as individual self-deception, historians should by now be skeptical of the excuse that "we never knew." Self-deception usually involves choices to screen out or discount disagreeable truths. Human beings are not passive automata wholly programmed by "society."

But Haskell ignored or dismissed a vast body of knowledge that reveals the ways in which class, gender, and social circumstances tend to guide our selective attention, control our mechanisms of avoidance, displacement, and projection, and shape the very cognitive style to which Haskell gives center stage. Sexism provides a telling example, since many American men are just beginning to realize the ways in which their language, culture, and daily conventions reinforce sexual inequalities. Sexism is seldom the result of conscious intent, but individual males benefit from the assumption that female disabilities are part of the natural order of things. Male hegemony has usually been accepted by women, who have redefined it in their own way but have found it difficult to create an alternative world. Class and racial hegemony can have similar effects, although Haskell's emphasis on conscious intention can blind us to the psychological realities we confront every day.

Historians' assumptions about human behavior are necessarily drawn from personal experience or from reading about the experiences of others. Most of us are probably aware of the complexity and ambiguity of our own intentions and may recall making choices that led to supposedly unintended consequences that we may still have intuited or dimly foreseen. Fiction offers endless variations on the themes of contradictory impulses, inner division, and the need for self-justification. Historians must engage in a dialectical process in which we strive to discover the "otherness" of the past while also making sense of past events from a present-day perspective. If we have empirical evidence that a group of historical actors believed they were advancing the interests of all humanity when they were actually promoting the interests of a special class, we can justifiably speak of self-deception. But my central concern in *The Problem of Slavery in the Age of Revolution* was not the conscious or unconscious motives of individuals but rather the social meanings and consequences of certain moral perceptions.

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From the time of the Mosaic Exodus, slavery and redemption have been extremely powerful paradigms involving the ultimate questions for both individual and collective life: the passage from present misery and degradation to a land of Canaan. Apart from their religious meanings, these paradigms are capable of being extended to a wide range of social experiences with oppression and liberation or of being confined to the historical sufferings of a specific people. According to Rousseau, "man is born free—and everywhere he is in chains." But since Rousseau, at least, there has always been a tension between such generalizing proclamations and attempts to dramatize the horrors of a special instance of human bondage.

For James Stephen, William Wilberforce, and the government leaders who deplored the African slave trade and who moved toward gradualist antislavery policies, it was essential to maintain a sharp distinction between the evils of the colonial slave world and the ostensibly free institutions that had been imperiled both by French tyranny and English Jacobinism. The constant comparisons in abolitionist literature between the agony of black slaves and the smiling, contented life of English husbandmen was not fortuitous. Abolitionists repeatedly reminded Britons that the Somerset decision of 1772 had outlawed slavery in England. At a time when many of the peoples of Europe were said to be "enslaved" by French despotism, it was crucial to define England as a "free" nation—both in the sense of having no slaves and of having successfully resisted foreign domination. With the growth of nationalism in the Napoleonic era, "freedom" increasingly signified membership in a nation that had resisted or thrown off foreign tyranny. When national leaders were perceived as the protectors of liberty in this collective sense, it was more difficult to accuse them of fostering various forms of domestic oppression.

If the existence of the slave colonies allowed England to define itself as free soil—much as communist countries enable the United States to define itself as the leader of the free world—they also helped specify the nature of freedom. The African slave trade defined, by negative polarity, the conditions necessary for consensual and acceptable labor transport. On the one hand, it was unacceptable for an employer to claim ownership of the person of an employee, to sell husbands apart from their wives, or children apart from their parents. But it was acceptable, on the other hand, to buy the labor of adults or children under conditions that led to the separation of families and that made a mockery of the worker's supposed consent. Labor conditions could be seen as the effects of market forces, not human intentionality.

IN DISCUSSING INTENTIONALITY, Haskell pointed to the absurdity of blaming Hank Aaron, when he hits a home run, for allowing the ball to strike a spectator on the head. The gap between Aaron's intention of hitting the ball and the consequence of injuring a spectator is too wide to be of any significance. Haskell was primarily concerned with the virtue or blameworthiness of individual moral choice. But, if the rules of a game were constructed in a way that ensured that many spectators

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would be hit on the head during every inning, and if it were assumed that this was simply one of the costs of the sport, we would consider the consequences culturally significant regardless of the conscious intentions of the players.

Haskell also argued that selectivity is inevitable, that limits of moral responsibility have to be drawn somewhere, and that it is therefore unfair to blame abolitionists for not attacking forms of exploitation closer to home. I should emphasize that I am not interested in blame but rather in understanding the moral ambiguities of history. But Haskell's argument reminds me of similar reasoning advanced by Serafim Leite, the historian of the Jesuits in Brazil. In praising Manuel da Nóbrega and other sixteenth-century Jesuits for protecting the Brazilian Indians from enslavement and mistreatment, Leite is forced to deal with the unfortunate fact that Nóbrega openly defended and encouraged the African slave trade. But, according to Leite, to accuse the Jesuits of injustice for favoring Indians is like condemning a person for founding a hospital for tuberculosis while ignoring the victims of leprosy. One has to draw the line somewhere. Yet selection is seldom neutral, either culturally or psychologically, as we can see in Leite's own illustrative choices of tuberculosis and leprosy. Indians were conventionally pictured as the victims of consumptive physical or social diseases introduced by whites; the African's dark skin and physiognomy were sometimes seen as a kind of leprosy, or as the result of a leprous disease for which the European bore no responsibility. Leite accepts the traditional distinction between the freeborn Indian and the enslaved African, who was accustomed to bondage and whose labor was essential for successful colonization.[10]

British selectivity, as I suggested in *The Problem of Slavery in the Age of Revolution*, must be understood in terms of historical context. The government's first interventions in the colonial labor system coincided with an urgent domestic problem of labor discipline and labor management—not yet the problem of an industrial proletariat but of an immense rural labor force that had been released from traditional restraints and controls but not deprived of the independence of preindustrial village culture. Many Britons, including abolitionists, felt ambivalent about the changes accompanying early industrialization. Tensions mounted between the advocates of hard-headed utilitarianism and the defenders of traditional paternalism or evangelical benevolence. The issue of slavery provided a meeting ground for these diverse groups and for members of different propertied classes who longed to ensure stability while benefiting from the economic changes underway.

"Because the slave system was both distinctive and remote," I wrote, "it could become a subject for experimental fantasies that assimilated traditional values to new economic needs. An attack on the African slave trade could absorb some of the traditionalist's anxieties over the physical uprooting and dislocation of labor . . . By picturing the slave plantation as totally dependent upon physical torture, abolitionist writers gave sanction to less barbarous modes of social discipline. For

[10] Serafim Leite, *História da Companhia de Jesus no Brasil*, 10 vols. (Rio de Janeiro and Lisbon, 1938–50), 2: 227, 246–47; 6: 350–51.

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reformers, the plantation offered the prospect of combining the virtues of the old agrarian order with the new ideals of uplift and engineered incentive. Abolitionists could contemplate a revolutionary change in status precisely because they were not considering the upward mobility of workers, but rather the rise of distant Negroes to the level of humanity . . . British antislavery provided a bridge between preindustrial and industrial values; by combining the ideal of emancipation with an insistence on duty and subordination, it helped to smooth the way to the future."[11] I have quoted at length from this passage because I have sometimes been interpreted as arguing that British abolitionism was a "screening device" designed to distract attention from metropolitan exploitation.[12] In actuality, I was trying to suggest a far more complex model in which the colonial plantation system served as a projective screen or experimental theater for testing the ideas of liberation, paternalism, and controlled social change that were prompted in part by domestic anxieties. As one might expect in a society as deeply divided as early industrial Britain, different audiences drew contradictory conclusions from the experiments in overseas reform. But it is difficult to deny that the abolition cause offered both national and local ruling elites an increasingly attractive opportunity to demonstrate their commitment to decency and justice.

In the passage quoted above, I was also concerned with the implications of sharply separating slavery from other kinds of coerced labor and social domination. It is noteworthy that even Thomas Clarkson, who retained much of the liberal spirit of the late 1780s and early 1790s, found nothing inequitable about coerced labor. Any state, he said, could legitimately use convicts to work in mines or clear rivers. What outraged Clarkson and other early abolitionists was the claim of personal proprietorship that justified arbitrary and unlimited authority. The slaveowner's claims contrasted sharply with those of the idealized British squire, whose authority was constrained by law and custom, and with the rights of the rising capitalist, who was content to purchase labor in the market like any other commodity.

Above all, the slave system came to epitomize an inherent and inescapable conflict of interest, a kind of warfare sublimated or suspended from the time the original captive was subdued. For a while, the more moderate abolitionists searched for means to ameliorate this conflict, hoping that an end to further slave imports, for example, would persuade masters to promote their slaves' welfare as part of their own long-term self-interest. But the continuing negative growth rate of the West Indian slave population seemed to show that the system itself was unreformable and would lead to eventual genocide. This impression was reinforced by the slaveholders' truculent resistance to missionaries, moral uplift, the abolition of Sunday markets, restrictions on the flogging of women, and other benevolent measures. The major goal of the British antislavery movement by the early 1820s was to create a natural harmony of interests between planters and black

[11] Davis, *Problem of Slavery in the Age of Revolution*, 466–67.
[12] See especially Drescher, "Cart Whip and Billy Roller," 4.

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workers, a relation similar to the assumed mutuality between British landlords and tenants.

In arguing that the antislavery movement mirrored the needs and tensions of a society increasingly absorbed with problems of labor discipline, I was not saying that such needs and tensions are sufficient to explain the emergence and ultimate direction of antislavery thought. While emphasizing the importance of class and social context, I specifically warned against "the simplistic impression that 'industrialists' promoted abolitionist doctrine as a means of distracting attention from their own forms of exploitation."[13] My main theme was that opposition to slavery cannot be divorced from the vast economic changes that were intensifying social conflicts and heightening class consciousness; in Britain, it was part of a larger ideology that ensured stability while acclimating society to political and social change.

Even in Britain, where the cause won significant support from the governing elites, abolitionist thought had both conservative and radical aspects. Some readers have focused exclusively on the first part of my argument, in which I claimed that the acts of selectivity by abolitionists "helped to strengthen the invisible chains being forged at home." But I also emphasized that abolitionism "bred a new sensitivity to social oppression," "that it provided a model for the systematic indictment of social crime," and that it "ultimately taught many Englishmen to recognize forms of systematic oppression that were closer to home."[14] To illustrate the radical potentialities of antislavery thought, I quoted from Friedrich Engels precisely because he showed how abolitionist perceptions and locutions had become universalized by the 1840s: even a resident alien, with no roots in the abolitionist movement, appropriated the language and perspective of Anglo-American abolitionists when he exposed the "slavery" of Manchester's working class. As early as 1817, when Wilberforce and his friends in Lord Liverpool's cabinet feared that England was on the verge of revolution, another radical alien pointed to the connections between the oppression of West Indian slaves and the oppression of England's poor. Iain McCalman has recently discovered that Robert Wedderburn, a Jamaican mulatto whose slave mother was born in Africa, edited a London periodical, *Axe Laid to the Root*, that called for a simultaneous revolution of West Indian chattel slaves and English wage slaves. Working with Thomas Spence, Thomas Evans, and other London radicals, Wedderburn popularized a plebeian antislavery rhetoric in the taverns and hayloft chapels of London's underworld.[15]

[13] Davis, *Problem of Slavery in the Age of Revolution*, 455.

[14] Davis, *Problem of Slavery in the Age of Revolution*, 455, 467–68. Drescher has greatly amplified these themes, which do not contradict my position, as he seems to think. No doubt I should have cited more varied examples of the linkage between denunciations of colonial slavery and wage slavery, and I was unaware of the language in petitions that Drescher has discovered. It is my intention to explore this subject in a succeeding volume on the "Age of Emancipation." Drescher does not seem to deny that Wilberforce, Stephen, Macaulay, Clarkson, Cropper, Buxton, and the other national leaders of the early period were unsympathetic to the wage-slavery argument, which they associated with their enemies.

[15] Iain McCalman, "Anti-Slavery and Ultra-Radicalism in Early Nineteenth-Century England: The Case of Robert Wedderburn," *Slavery and Abolition*, 7 (September 1986): 99–117. Although

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Reform causes often serve opposing or contradictory functions as innovative doctrine is co-opted by different social groups. As I have already indicated, slavery and emancipation have long been extraordinarily complex paradigms capable of almost infinite extension to both material and spiritual states. Even in the 1820s, antislavery agitation led American radicals like Langton Byllesby and Thomas Skidmore to the conclusion that black slavery was not only the quintessential American crime but that it revealed deep structural flaws allowing a fortunate few to live off the labor of the so-called free majority.[16] Yet, when radical American reformers later contended that the wage system was slavery, that conventional marriage was slavery, and that submission to any government using coercion was slavery, their rhetoric surely diluted the charge that Negro slavery in the South was a system of exceptional and intolerable oppression. Frederick Douglass was outraged in 1843 when his white abolitionist colleague John Collins asserted that antislavery was "a mere dabbling with effects" and that tolerating private ownership of land was worse than enslaving human beings. As Christopher Lasch has observed with respect to our own time, the language of radical protest was impoverished when it was appropriated by fat people, short people, old people, and other such groups who claimed that they were as much oppressed as racial minorities. "Since interest-group politics invites competitive claims to the privileged status of victimization, the rhetoric of moral outrage becomes routine, loses its critical edge, and contributes to the general debasement of political speech."[17] Likewise, an indispensable term such as "holocaust" becomes trivialized when it is extended metaphorically to every atrocity or instance of ethnic oppression.

The concept of hegemony is easily discredited by misconstruction or misunderstanding—by attacking the argument, for example, that a discrete capitalist class imposed a form of false consciousness on a passive populace, duping people with antislavery propaganda designed to divert attention from the women and children in the mills and mines. It is now clear that, by the early 1830s in both England and the United States, the abolitionist movement had attracted significant support from artisans and other skilled workers; that in England the "pressure from without" ran ahead of the elite antislavery leadership, embarrassing Thomas Fowell Buxton in his negotiations with government ministers; and that a few reformers moved from an apprenticeship in the abolitionist campaign to more radical activism as Chartists or labor reformers.[18]

Wedderburn dedicated his autobiography to Wilberforce, it is clear that Wilberforce abhorred the ideology and culture that Wedderburn represented. Nevertheless, disguised as a "stranger" and equipped with a Bible, Wilberforce visited Richard Carlile in prison and attempted to convert him to Christianity; Carlile reported that Wilberforce also talked to the imprisoned Wedderburn and declared the black to be an "honest and conscientious man." John Pollock, *Wilberforce* (New York, 1977), 258; McCalman, "Anti-Slavery and Ultra-Radicalism," 113.

[16] Sean Wilentz, *Chants Democratic: New York City and the Rise of the American Working Class, 1788–1850* (New York, 1984), 164–67, 183–90.

[17] Waldo E. Martin, Jr., *The Mind of Frederick Douglass* (Chapel Hill, N.C., 1984), 28; Christopher Lasch, "The Great American Variety Show," *New York Review of Books*, 2 February 1984, 36.

[18] For a discussion of the pressure exerted on Buxton and the parliamentary abolitionists, see Davis, *Slavery and Human Progress*, 195–202. Drescher's *Capitalism and Antislavery* emphasizes the broad-based, popular character of the antislavery movement. For abolitionist Chartists, see Betty Fladeland, "Our

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But these facts in no way invalidate the hegemonic argument when it is properly understood. Hegemony, as Eugene D. Genovese has written, implies no more than the ability of a particular class to contain class antagonisms "on a terrain in which its legitimacy is not dangerously questioned."[19] Ideological hegemony is a process that is never complete or total; it can be understood in different ways by opposing groups or classes as long as it limits the terms of debate, heads off more fundamental challenges, and serves to reinforce the legitimacy of the ruling groups and existing order. Seymour Drescher, who scornfully rejects the hegemonic thesis, lends it invaluable support in his description of the mass political mobilization by abolitionists that was "virtually unopposed in most areas." A perfect example of ideological hegemony can be seen in Drescher's claim that "Parliament's assessment of property claims in persons residing in the metropolis appears to have been congruent with that of the Irish chimney sweep." In France, in sharp contrast, "antislavery was clearly distinguished by an inability to combine a stable élite leadership with a mass appeal."[20] Obviously, British antislavery agitation had different meanings in 1814, 1833, and 1838, and a detailed analysis would be required to show the degree to which abolitionism stabilized or destabilized Britain's social and political order at a particular time. But a few preliminary points can be made in response to the common view of an expansive, one-directional surge of democratic consciousness.

No doubt many British workers empathized with colonial slaves and understood abolitionist principles in ways that would have deeply troubled Wilberforce, Buxton, and Zachary Macaulay. But rank-and-file abolitionists could not escape the fact that the governing classes succeeded in appropriating the cause and defining the terms of the debate. Britain's landlords, merchants, and manufacturers showed by their behavior that there were varieties of exploitation that would no longer be tolerated in England or on the high seas, and that there were forms of labor, even in the distant colonies, that would have to be brought more in line with metropolitan standards. This affirmation of moral standards helped legitimate both the existing system of class power and the emerging concept of free labor as an impersonal, marketable commodity. The emancipation act of 1833 gave assurance to Britons of various classes that there were limits to the rapid socioeconomic changes taking place: workers could not literally be reduced to chattel slavery; owners of even the most questionable form of private property could not be deprived of their capital without generous compensation.

Although the politics of slave emancipation were extremely complex, the act of 1833 fostered the illusion that the newly reformed Parliament had become an almost democratic assembly that would respond to the voice of a moral majority. The succession of antislavery victories and official commitments, beginning with the order-in-council of 1805 restricting the slave trade to conquered colonies,

Cause Being One and the Same': Abolitionists and Chartism," in *Slavery and British Society, 1776–1846*, James Walvin, ed. (Baton Rouge, La., 1982), 69–99.

[19] *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1974), 26.

[20] Drescher, *Capitalism and Antislavery*, 46–47, 53, 75.

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vindicated trust in the government's basic sense of justice. It is no wonder that, when various British groups wanted to dramatize their own oppression or lack of freedom, they complained that their condition was at least as bad as that of West Indian slaves. Defenders of colonial slavery had opened this door, and their argument implied two propositions: first, to receive attention, one had to meet the "slavery test" by enumerating horrors equivalent to those in abolitionist literature; second, because Parliament and the middle-class public were attuned to this language, the same techniques that had persuaded Parliament to bestow liberty on West Indian slaves would also bring freedom and justice at home.

In effect, the antislavery radicals were addressing the governing classes as follows: wage labor under present conditions leads to even worse misery than chattel slavery; because you responded to moral arguments in abolishing the slave trade and in freeing the colonial slaves, you should now relieve the distress of England's poor. But this reinforcement of ruling-class standards is precisely what is meant by "ideological hegemony." Denunciations of "wage slavery" were a way of expressing outrage and resentment over working conditions in industrial Britain and America. But as Christopher Lowe pointed out, there could be no lower standard than to ask that free laborers be treated better than slaves.[21] Everyone knew that white workers were not really slaves. The analogy, whatever its emotive power, invited a rhetorical response celebrating the benefits of the market and the inestimable privilege of being free to change employments. The dichotomous terms of this debate forced radicals to prove that, in some fundamental respects, wage earners were no freer or better off than slaves.

There can be no doubt that abolitionism contributed to more radical kinds of social criticism. Especially in the United States, where slavery was abolished in a cataclysm of violence, radical labor leaders and socialists found that parallels between black and white slavery retained their resonance well into the twentieth century.[22] But analogies with chattel slavery may also have retarded the development of a vocabulary that could depict more subtle forms of coercion, oppression, and class rule. To be a free worker was to be as unlike a Negro slave as possible. Most opponents of slavery equated unjust domination with a legalistic concept of property rights in human beings. This absolutist approach often made it difficult to distinguish the forms of domination concealed by voluntary contracts and the "bundle of powers" that could be exercised over nominally free workers.

HASKELL SEEMED PRIMARILY CONCERNED WITH the *moral* status of individual acts of benevolence and charity, such as our going to the aid of a starving stranger. He evidently feared that any links with class interest would inevitably tarnish the virtue of humanitarian acts by reducing them to lowly motives. His thesis allowed a place for class interest but minimized its importance when compared with his own alternative explanation: "Whatever influence the rise of capitalism may have had

[21] Christopher Lowe, "Ideology, Hegemony and Class Rule" (Yale Graduate School seminar paper, 1985).

[22] Barry Herbert Goldberg, "Beyond Free Labor: Labor, Socialism and the Idea of Wage Slavery, 1890–1920" (Ph.D. dissertation, Columbia University, 1979).

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generally on ideas and values through the medium of class interest, it had a more telling influence on the origins of humanitarianism through changes the market wrought in *perception* or *cognitive style*."[23] Aside from the point that changes in perception and cognitive style are often closely associated with changing class interests, I fail to see why aiding a starving stranger is more praiseworthy when it is the psychological by-product of market forces that may also encourage moral callousness and bring starvation to the unemployed. Indeed, one can argue that we have even less reason to admire Haskell's humanitarians, since their altered perceptions seem to be the mechanistic and behavioristic result of what he himself terms "market discipline." [24]

I see no need, however, to fall into this trap of reductionism and counter-reductionism. Few historians would maintain today that abolitionists were hypocrites who consciously exploited humanitarian sentiments for ulterior aims. Few historians would argue that abolitionism was simply a spontaneous eruption of virtue, wholly unrelated to the rise of modern capitalism and the concomitant redefinition of property, labor, and contractual responsibilities. The abolitionists were neither otherworldly saints nor the agents of a capitalist conspiracy. Whatever their virtues or shortcomings, they have been vindicated by history: morally, they were right. But, like their opponents, their private moral and personal needs had public consequences that reinforced or altered relationships of power. Haskell confused the issue by conflating the hegemonic argument with the origins of the antislavery movement. The movement had various origins and was supported by different groups for different reasons. Only gradually did the British movement become hegemonic in the sense of promoting an alliance of various religious, social, and political blocs that succeeded in overriding the interests of West Indian merchants, landowners, and their conservative allies. It was precisely because the antislavery alliance could convincingly present itself as disinterested and high-minded that it bestowed moral legitimacy on the reformed Parliament, British society, and the British character.

Haskell recognized a connection between British capitalism and antislavery thought, but he posited an original and ingenious theory about the effects of the market on the understanding of causation, promise-keeping, foresight, and the conventions defining personal responsibility. Haskell's reasoning deserves more careful consideration than I can give here, but I will offer a few preliminary observations.

It should be noted that Haskell applied extremely rigorous standards to the hegemonic theory, demanding that it explain, in class terms, the origin and rise of British abolitionism. In other words, Haskell seemed to be looking for a strong causal link between some element of capitalism and the appearance of abolitionism. Any link based on class, he argued, must ultimately rely on intentionality, for which there is no empirical justification. When Haskell turned to his own alternative thesis, however, he retreated from the demand for a strong causal link.

[23] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 1," 342.
[24] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 1," 342.

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His market mechanisms and recipe knowledge supply only one of various unnamed preconditions for the humanitarian sensibility and, more specifically, for the rise of abolitionism. This weak connection has no predictive power—that is, Haskell did not suggest that merchants involved in a world market were more likely to become abolitionists than were merchants confined to a traditional village clientele. But this very softness shields Haskell from serious empirical attack. For example, there can be no doubt that the market tended to teach people to keep their promises and attend to the remote consequences of their acts. But such lessons were especially useful for slavetraders, who were in the vanguard of long-distance commerce and who had to calculate costs and risks against long-delayed payment. The British slave trade flourished and indeed rose to its peak during the very period when Haskell's capitalist market was becoming triumphant. It is clear that a market mentality enabled many merchants to scorn older, paternalistic notions of responsibility and to justify virtually any practice that was good for business. Indeed, one can argue that market values tended to shrink the scope of effective human agency, as economists ascribed the misery of the poor to the effects of immutable economic laws and as people became aware of the interconnections of a global economy that fractionalized individual responsibility beyond measure. The knowledge that our economic acts are related in some way to most of the world's crime and oppression can blunt any sense of complicity. Even so, Haskell can claim that nothing is proved by the failure of slavetraders to become humanitarians. The market did not breed humanitarians but was a precondition that enabled certain kinds of people to become humanitarians.

The same reply can be given, though with much less force, to certain counterexamples. Antislavery opinion emerged in various societies including those of Virginia and North Carolina, France and Brazil. These societies were involved in international markets that required a degree of rational calculation and long-range planning. But only in industrializing Britain did this opinion crystallize into a national consensus that received official sanction. If the British pattern had appeared in market-oriented nations that had not yet shifted to a wage-labor economy, Haskell's thesis would appear more plausible. Given Haskell's criteria, a strong antislavery movement should have emerged in Holland, which was certainly involved in mercantile capitalism, in long-distance commerce, in world markets, and in complex banking and credit. Surely, the Dutch learned to attend to the remote consequences of their actions, and there must have been as many potential humanitarians per capita in the Dutch population as in Britain or the United States. Yet, despite repeated prodding from British abolitionists, the Dutch remained stolidly indifferent to the whole abolitionist campaign. Antislavery ideas excited virtually no popular interest. Before 1840, only twelve pamphlets were published in Holland on the issue of slave emancipation. It was not until 1853 that Dutch liberals succeeded in founding an antislavery society, but, according to Peter Emmer, the minutes of the annual meetings "show a complete lack of enthusiasm among its members: only seventeen to twenty of them attended." In 1863, long after slavery had been abolished in the British, French, and Danish colonies, the

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Dutch government emancipated Holland's colonial slaves in an almost perfunctory gesture.[25]

The market theory also fails to account for the large number of women who were involved in the abolitionist campaigns of Britain and America. Clearly, women as a group were relatively removed from the kind of liberating capitalist market Haskell had in mind; married women were not even permitted to make contracts in their own name. They were not subject to a market discipline that rewarded "impersonality" and "efficiency," to use Haskell's terms, or that "taught them to attend to the remote consequences of their actions."[26] Yet women were strongly represented among the petition-gatherers, the local organizers, and the writers and distributors of antislavery literature. Haskell ignored the whole pre-Romantic and Romantic cults of sensibility and domesticity that were pitted against the brutality and insensitivity of the marketplace but that gave a powerful impetus to the abolitionist crusade. One can argue that the cultivation of empathy, which was obviously central to all humanitarian reform, arose in large part as a reaction to what Haskell termed the "nineteenth-century folklore about avaricious landlords and piratical factory owners . . . the metahistorical imagery of a class of me-first bourgeois individualists."[27]

HASKELL MADE SOME EXTREMELY INTERESTING POINTS about the effects of the market on cognitive style and recipes for action. But I would shift attention from the origins of individual humanitarian sensibility to the way that market values highlighted the evils of Negro slavery. Haskell failed to see that thinking causally, keeping promises, learning to calculate and compute, and taking responsibility for the remote consequences of one's actions is precisely the kind of behavior that slavery proscribed. How could a slave be expected to keep all promises or make responsible choices? Slavery stood in direct opposition to the virtues inculcated by the market, the virtues that English employers and ratepayers wished to instill in the English working class. This line of reasoning brings me back to class interests and hegemony—not to any rigid or mechanical notion of social control but to the broad moral, political, and cultural transformations that accompanied the triumph of capitalism.

[25] P. C. Emmer, "Anti-Slavery and the Dutch: Abolition without Reform," in *Anti-Slavery, Religion, and Reform: Essays in Memory of Roger Anstey*, Christine Bolt and Seymour Drescher, eds. (Folkestone, Kent, 1980), 80–94.

[26] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 2," 550, 551.

[27] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 2," 549.

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AHR Forum

The Relationship between Capitalism and Humanitarianism

JOHN ASHWORTH

SINCE AN OBVIOUS TEMPORAL CORRESPONDENCE exists between the development of capitalism and the rise of humanitarianism, historians are understandably reluctant to believe that there is no causal connection. The problem, however, is to explain, indeed to theorize, this connection. The two-part article recently published in this journal by Thomas L. Haskell is an attempt to do just that.[1] One can hardly fail to be impressed by the authority that Haskell brings to the subject. The wide-ranging knowledge, the variety of sources consulted, and the rigor of the theoretical analysis make his article a landmark in the historiography of antislavery. I have reason, nevertheless, to be skeptical about the interpretation he offers and, perhaps more important, reason to question the sociological assumptions on which it rests.

It may be helpful to begin with a brief review of the historiography or at least parts of it. First, there was the Whig view of the abolitionists. It was probably not very different from the abolitionists' view of the abolitionists. The emphasis was on progress; waves of humanitarian sentiment came lapping onto the shores of Britain and then the United States as part of the divinely ordained scheme of things. Quaint though some of their notions may seem today, Whig historians did recognize that morality and values were subject to change over time. At this point, enter Eric Williams with his attack on the abolitionists and his reduction of humanitarianism to a simple reflex of self-interest. This view has been effectively demolished by, among others, Howard Temperley and Seymour Drescher, who have shown that good cause exists for doubting whether abolition promoted Britain's economic interest. But, as Temperley has pointed out, the Williams view does at least have the merit of connecting the rise of capitalism to the emergence of the humanitarian movement. His view also explains the contrast between the abolitionists' condemnation of slavery and their tolerance of, or even enthusiasm

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[1] Thomas L. Haskell, "Capitalism and the Origins of the Humanitarian Sensibility," 2 parts, *AHR*, 90 (April 1985): 339–61; (June 1985): 547–66.

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for, the system of wage labor then emerging. It seems to be difficult, if not impossible, however, to demonstrate the existence of a direct economic interest in the abolition of slavery.[2] But David Brion Davis, against whose work much of Haskell's article is directed, has concluded that capitalism brought to prominence a bourgeois class whose interest it was to attack slavery. This was not a simple financial interest that can be calculated in pounds, shillings, and pence but a much broader one, involving ideas about labor discipline and unemployment. With this more subtle notion of self-interest, Davis was able to explain, without resort to crude charges of hypocrisy, what he took to be the abolitionists' lack of concern with, for example, factory conditions. The abolitionists were responding to their own class needs. Davis also argued that they were not generally conscious of these limitations on their world view: they believed their ideals were universal rather than partial.[3]

This interpretation allowed Davis, like Williams, to recognize that values and morality are subject to change. It also maintained the spotlight on the limitations of the abolitionists, their special concern with slavery. In other respects, Davis has advanced beyond Williams, and Haskell justly praised him for not charging those who opposed slavery with conscious deception. Yet, according to Haskell, Davis has fallen into a different trap. Haskell's principal criticism of Davis maintains that he was wrong to try to connect abolitionism and capitalism via class interest.

This criticism raises an important theoretical point. Haskell's argument, I believe, is misdirected. Scholars now agree that it is a mistake to charge the abolitionists with conscious deception or hypocrisy. And Haskell has spent a considerable amount of time showing that it is impossible to claim that unconscious intention was present, either. For how can historians establish an unconscious intention? If an intention is unconscious, the individual is presumably not aware of its existence. How can the historian ever hope to demonstrate self-deception and unconscious intention? Showing conscious intention is difficult enough. Davis's argument is in fact incoherent. Haskell concluded, "To say that a person is moved by class interest is to say that he *intends* to further the interests of his class, or it is to say nothing at all."[4]

Although Haskell's conclusion sounds plausible enough, it must be resisted. Davis has only himself to blame for talking about self-deception and thus opening himself to Haskell's criticism. Davis ought to have focused on "false consciousness," the notion that the awareness of historical actors is incomplete, with the result that they misperceive the world around them. Historians are often, understandably,

[2] Thomas Clarkson, *A History of the Rise, Progress and Accomplishment of the Abolition of the African Slave Trade*, 2 vols. (London, 1808); W. P. Garrison and F. J. Garrison, *William Lloyd Garrison, 1805–1879*, 4 vols. (New York, 1885–89); Eric Williams, *Capitalism and Slavery* (Chapel Hill, N.C., 1944); Seymour Drescher, *Econocide: British Slavery in the Era of Abolition* (Pittsburgh, Pa., 1977); Howard Temperley, "Capitalism, Slavery and Ideology," *Past and Present*, 75 (May 1977): 94–118; Howard Temperley, "The Ideology of Antislavery," in *The Abolition of the Atlantic Slave Trade: Origins and Effects in Europe, Africa and the Americas*, David Eltis and James Walvin, eds. (Madison, Wis., 1981), 21–35; Howard Temperley, "Eric Williams and the Abolitionists: The Birth of a New Orthodoxy," in *British Capitalism and Caribbean Slavery*, Stanley Engerman and Barbara Solow, eds. (Cambridge, 1987).

[3] David Brion Davis, *The Problem of Slavery in the Age of Revolution, 1770–1823* (Ithaca, N.Y., 1975).

[4] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 1," 347.

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reluctant to employ this concept, perhaps because it smacks of condescension toward the past, perhaps because the word "false" is insufficiently nuanced. Yet, it seems to me, we cannot do without the concept. Otherwise, we are limited to the understanding of events that contemporaries possessed. The most important factors to them will have to be the most important for us; what they are unaware of, we will be unable to discuss. But Haskell's objection to the idea of self-deception is quite justified. The way out of this impasse lies, quite simply, in a recognition that society rather than the individual generates false consciousness. In the words of one Marxist theorist, "it is not the subject that deceives himself, but reality which deceives him." When Marie Antoinette told the peasants of Paris (never mind that the story is probably apocryphal) to eat cake when there was no bread, she was not deceiving herself in thinking that they could afford it. Rather, the nature of her involvement in society obscured from her the realization that peasants could not in fact afford cake. False consciousness, which human beings all possess to a greater or lesser degree, is not a matter of self-deception.[5]

One can say that a person is moved by certain ideals that have grown out of class interest. This knowledge may or may not be reflexive; the individual may or may not be aware of the relationship between ideals and self-interest. History often illustrates the proposition that actors are not always conscious of the forces that operate on them. It is clearly quite possible to argue that an individual is moved by class interest without showing that the individual intends to further those interests either consciously or unconsciously.

I do agree with Haskell that current interpretations that point to class interest as the major causal factor are open to serious criticism.[6] Haskell of course answered his main question, the relationship between capitalism and humanitarianism, by reference to the market and its multifarious effects. Before considering this view, let us first note that, in what it seeks to do, Haskell's argument marks a retreat from certain issues. Davis's view, despite its limitations, did tackle this crucial point about the selectivity of the abolitionists, their concern with chattel slavery rather than, as some contemporaries put it, "wage slavery." Haskell has nothing to say on this selectivity except for his demonstration that there was no conscious hypocrisy on the part of the abolitionists and his (powerful) argument against the notion of self-deception. Granting that neither conscious hypocrisy nor self-deception was present makes the question of the abolitionists' selectivity of concern more, not less, urgent. Haskell introduced an interesting analogy here. If, in a hundred years'

[5] M. Godelier, "System, Structure and Contradiction in Capital," quoted in Nicholas Abercrombie, *Class, Structure and Knowledge* (Oxford, 1980), 77. It is evident that no individual can ever attain an entirely "true" consciousness, just as the person burdened with a consciousness that is utterly "false" would be incapable of any rational behavior or thought. In other words, we are dealing with a continuum rather than the dichotomy that is perhaps implicit in the term "false."

[6] The major problem arises not from the concept of intentionality but from that of interest. On this subject, Haskell's words seem to me important enough to bear repeating. He referred to "the glib assumption so characteristic of modern scholarship that a person's 'interests' are readily identifiable and constitute a complete explanation of his conduct." The problem is that "the term is utterly elastic" so that "there is no human choice that cannot be construed as self-interested." To this, I would add only that this is an especially severe problem for scholars of a Marxist persuasion. Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 1," 351.

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time, everyone were a vegetarian, would the many twentieth-century reformers who ignore this question today be deceiving themselves? Indeed, they would not, but the analogy is perhaps better than Haskell realized. If that moment were ever to arrive, a vital question for historians studying the twentieth century would be why its peoples were selective in their concerns. In other words, the historians of the future would want to know why so many of us, unlike their own contemporaries, were indifferent to this reform. Does Haskell assume that he is not entitled to go beyond the actors' own understanding of their ideas? Certainly, when he finds abolitionists who speak of what he regards as the crucial factor (the market), he seems to think these statements are especially significant.[7] Yet, if one had asked abolitionists why they were abolitionists, they would have talked a good deal about God and righteousness, a good deal about the evil effects of slavery, and very little about the spread of the market. In short, something very like the Whig view.

It is important theoretically to decide whether the abolitionists' understanding of their own ideas is definitive or not. If it is, historians must speedily resurrect the Whig view. But if it is not, we are entitled to ask Davis's question again: why were abolitionists selective in their concerns? Search Haskell's article as we may, we will find nothing that helps answer this question. All the links postulated between capitalism and humanitarianism via the market might just as easily have operated to make the conscientious reformer hostile to wage labor, too. In this connection, we can also note that abolitionists in the United States and Britain tended to be involved in many other reform causes. Why did they accept wage labor? Haskell quoted an abolitionist, O. B. Frothingham, saying that slavery, unlike poverty, was susceptible to human control. We need neither doubt Frothingham's sincerity nor accuse him of self-deception to ask why it was that he and others reasoned in this way. Haskell, far from answering this question, seems to doubt whether it should even be posed.[8]

HASKELL PRESENTED HIS EXPLANATION WITH ADMIRABLE LUCIDITY in one of his concluding paragraphs: "What, then, did capitalism contribute to the freeing of the slaves? Only a *precondition*, albeit a vital one: a proliferation of recipe knowledge and consequent expansion of the conventional limits of causal perception and moral responsibility that compelled some exceptionally scrupulous individuals to

[7] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 2," 564.

[8] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 2," 557. Historians have recently revised the assumption that abolitionists were uninterested in reforming institutions and practices that were closer to home. Many maintained an interest in a wide range of issues, and some were concerned with the conditions of the laboring poor in their own societies. Nonetheless, for all but a small minority, there was a characteristic belief that the relation between capitalist and worker either was, or could be made, harmonious; by contrast, the relation between slave and slaveholder by contrast, could not. See Betty Fladeland, *Abolitionists and Working-Class Problems in the Age of Industrialization* (London, 1984); Jonathan A. Glickstein, "'Poverty Is Not Slavery': American Abolitionists and the Competitive Labor Market," in *Antislavery Reconsidered: New Perspectives on the Abolitionists*, Lewis Perry and Michael Fellman, eds. (Baton Rouge, La., 1979). There are many comments by American abolitionists on the virtues of wage labor. See, for example, *Liberator*, 1 January 1831, 24 December 1841, 19 and 26 March 1847, 9 July 1847, 1 October 1847; *The Letters of William Lloyd Garrison*, 4 vols., Walter M. Merrill, ed. (Cambridge, Mass., 1971), 2: 167; *Emancipator*, 10 October 1839, 31 December 1840; Theodore Parker, *The Slave Power* (1916; rpt. edn., New York, 1969), 63, 116.

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attack slavery and prepared others to listen and comprehend. The precondition could have been satisfied by other means, yet during the period in question no other force pressed outward on the limits of moral responsibility with the strength of the market."[9] That is, the market enhanced causal understanding, deepened the sense of responsibility, and thus spurred the reformer into action.

Of course, Haskell was aware of the other effects of capitalism. The market could also encourage the atavistic pursuit of self-interest. As he himself commented, "both friends and foes of capitalism often read into technical analyses of wage and price movements a very simple message: since the laws of supply and demand automatically transmute each individual's self-interest into the greater good of the greater number, no one need be concerned with the public interest."[10] This message was undoubtedly received loud and clear by many nineteenth-century Europeans and Americans. The market in this case had contradictory effects. Surely, to substantiate his argument, Haskell must show that the effects he wants to emphasize were actually greater than the opposite ones. If one demonstrates that factor f tends both to produce and to prevent result r, then to make f a significant cause of r, one must show that the productive effects are greater than the preventive ones. Nowhere did Haskell do this. He did not even acknowledge the difficulty. Yet it may be that he sensed the problem. Certainly, he was at pains to emphasize the positive effects of the market. The language he used to refer to the negative ones is revealing. He refers to "the rich nineteenth-century *folklore* about avaricious landlords and piratical factory owners" and to "the *metahistorical* imagery of a class of me-first bourgeois individualists displacing a feudal aristocracy" (emphasis added). More explicit is the remark that "it is easy to forget . . . how moderate, in the long perspective of human history, the capitalist's license for aggression really is."[11] Whether one agrees or disagrees with this statement, it hints at a major problem in Haskell's argument, a problem that was not adequately identified, let alone resolved.

Let us for the moment concede that Haskell was correct in emphasizing these positive effects of the market. Suppose that more individuals do become aware of the importance of their actions and the consequences that will flow from them. Will this be conducive to reform movements such as abolitionism? Haskell cited the case of John Woolman, the early Quaker abolitionist. In an essay in 1746, *Some Considerations on the Keeping of Negroes*, Woolman suggested that custom might have to be breached and people treat Negroes as they would themselves wish to be treated. He made it his responsibility to point out to his readers the extent to which they were implicated in slavery and the slave trade. Woolman argued that both geographical and temporal remoteness were irrelevant. Woolman recognized (as few did until the nineteenth century) "the causal relationship that exists in market

[9] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 2," 563.
[10] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 2," 549.
[11] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 2," 549. It is perhaps necessary to add that this is separate from the (important) question of who the reformers actually were. Haskell quite legitimately disclaimed any intention of answering this question. I too am concerned with the overall societal effect.

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societies between supply and demand." Haskell quoted Woolman at some length: "'Whatever nicety of distinction there may be betwixt going in person on expeditions to catch slaves, and buying those with a view to self-interest which others have taken, it is clear and plain to an upright mind that such distinction is in words, not in substance; for the parties are concerned in the same work and have a necessary connection with and dependence on each other. *For were there none to purchase slaves, they who live by stealing and selling them would of consequence do less at it*'" (emphasis in original).[12] Haskell concluded that a man "attentive to the remote consequences of his acts in business and familiar with the intricate web of mutual dependencies that the market establishes between buyers and sellers" was naturally "among the first to see the seemingly civilized and law-abiding slaveowner as engaged in essentially 'the same work' as the barbaric slave stealer."[13] Woolman provides good evidence for Haskell. Unfortunately, it is negated by the case of Daniel Defoe, whom Haskell also cited but without seeing the damage it did to his thesis. Although Defoe "conceived of the slave trade as part of a network of cause-and-effect relationships, he drew from this in 1713 the conclusion that the slave trade was indispensable to England's prosperity." Defoe represents one who appreciated the role of the market in producing intricate patterns of interdependence but who, instead of arguing for abolition, drew the opposite conclusion: one must not tamper with its workings. This stance was anything but rare. In the United States, for example, it was adopted time and time again. To blunt the force of the abolitionist attack on slavery, proslavery advocates pointed to "the intricate web of mutual dependencies" established by the market between buyers and sellers. Too many people, too many interests, it was said, were dependent on slavery. So the complexity produced by the market could have a paralyzing effect. Once again, the consequences are contradictory; once again, Haskell supplied no reason for believing the tendencies that he emphasized were not offset by the opposite ones.[14]

In common with many other writers on the subject, Haskell seemed to define capitalism very much in terms of the market. Let us consider for a moment the South. According to Haskell, the market promoted new levels of scrupulous attention to ethics. An unprecedentedly high standard of conscientious performance was encouraged. The South had always been connected very firmly with the international market. From 1820 onward, Southerners were major suppliers of raw cotton to Europe and particularly to Britain. They were involved in the market and, at the same time, required by Southern honor to maintain high "levels of scrupulosity in fulfilling ethical maxims."[15] Yet no powerful humanitarian movement took place in the South. Not only antislavery but all the other reform movements that flourished in the North and in Britain were much weaker there.

[12] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 2," 565.
[13] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 2," 566.
[14] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 2," 564–66.
[15] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 2," 555.

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Why was this? It is not easy to see how Haskell's thesis can explain this anomaly.[16]

Finally, an even greater difficulty confronts his argument. Whatever the deficiencies of the Whig view of history, which saw the abolitionists as representatives of a new and superior moral order, it did at least have the merit of recognizing that morality itself, the notion of what is right and wrong, has not been a constant in human history. Changes in morality are not the same as a judgment whether an evil is or is not bearable: I am concerned here with the prior question of whether a practice is an evil at all. This distinction is important in the case of slavery. At times, Haskell assumed that the problem of slavery is analogous to that of the starving stranger today: we all know the situation is bad, but we lack a feeling of responsibility for it and a recipe to end it. Thus he referred to "the miseries of the slave, which had always been recognized but which before the eighteenth century had possessed the same cognitive and moral status that the misery of the starving stranger in Ethiopia has for us today."[17] But is this correct? Did people always recognize that slavery was an evil? There is every reason to doubt it. Davis's earlier volume, *The Problem of Slavery in Western Culture*, clearly demonstrates that many defenses of slavery were offered and that, while some individuals and groups were convinced that the enslavement of human beings was an evil, others held the opposite view, and a third group had an ambivalent attitude. Davis concluded an excellent analysis of antiquity's view of slavery with the words: "For some two thousand years men thought of sin as a form of slavery. One day they would come to think of slavery as sin."[18]

It would be wrong to imply that Haskell was unaware of the evidence for changing morality. As he himself observed, "Although its morality was often questioned before 1750, slavery was routinely defended and hardly ever condemned outright, even by the most scrupulous moralists."[19] This statement sits rather awkwardly with the analogy of the starving stranger. Is poverty in the third world "routinely defended and hardly ever condemned outright"? The core of the problem is an ambiguity in Haskell's argument. Was he arguing that a change in values occurred so that what was once believed acceptable ceased to appear so? When he discussed earlier attitudes to slavery, that was the impression we were given. Or was he instead emphasizing a continuity of values but a discontinuity of technique and cognitive style? That was the impression offered when the starving stranger was being considered. A possible way out of this impasse would be to argue that a change in technique can generate a change in values, so that, having found a recipe or perceived a causal connection, human beings then find intolerable an evil they had previously been prepared to tolerate. Although this may seem the most sophisticated position to adopt, it tends toward reductionism.

[16] On Southern honor, see Bertram Wyatt Brown, *Southern Honor: Ethics and Behavior in the Old South* (New York, 1983).

[17] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 1," 359.

[18] David Brion Davis, *The Problem of Slavery in Western Culture* (London, 1966), 90. Of course, there were good reasons why people were slow to condemn slavery. As Davis noted, "if slavery were an evil and performed no divinely appointed function, then why had God authorized it in Scripture and permitted it to exist in nearly every nation?"; Davis, *ibid.*, 91–92.

[19] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 1," 339.

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For it is clear that the relationship can easily be reversed. Having come to find an old practice intolerable, people look for a recipe to end it or discover the extent of their complicity in it. To the extent that he assumed that the process is one-way, Haskell reduced values to technique and recipe knowledge. Again using "technology" in the broadest sense, so that it includes cognitive style, recipe knowledge, and technique, Haskell offered us an analysis that comes perilously close to technological determinism.

So, there are many reasons why historians should be reluctant to accept Haskell's thesis. First, the market has contradictory effects on a sense of responsibility, and, while Haskell realized this, he did not justify his own emphasis on its positive ones. Second, even if the market makes individuals conscious of causal processes in a new way, this consciousness may serve to deter rather than encourage reform. Third, even if the reformer were encouraged by the market, we would be no nearer an answer to the question Eric Williams posed: why the selectivity of concern; why some reforms rather than others? Why reject slavery but tolerate wage labor? Fourth, no conceptual space is allotted to the possibility that changes in values may precede and even cause the changes in cognitive style and technique that Haskell outlines. Fifth, difficulties occur with certain geographical regions such as the South. This is not to deny that a market was one of the necessary conditions for the rise of humanitarianism; capitalism cannot exist without markets, and it would not be surprising if there were some connection between markets and humanitarianism. But I shall argue that the connection is utterly different from the one Haskell suggested.

AT ONE POINT IN HIS ARTICLE, HASKELL INTRODUCED AN ANALOGY of oxygen and fire: "Since capitalism [For this, read "markets," since for him they are the same thing] supplied only a precondition, no one need be surprised that the subsequent history of capitalist societies has not been greatly distinguished by humanitarian achievements. The argument presented here is not that markets breed humane action but that in the particular historical circumstances of late eighteenth-century Anglo-American culture the market happens to have been the force that pushed causal perception across the threshold that had hitherto made the slaves' misery (and much other human suffering) seem a necessary evil. One would no more expect markets continually to elevate the morality of the population than one would expect oxygen—in the absence of which ignition cannot occur—always to produce fire. Then, too, there is reason to fear that still another face of the market has prevailed in the later stages of capitalism, one far less supportive of the humanitarian sensibility."[20] This passage is highly problematic. First, Haskell recognized that the market does not necessarily and indeed has not since the mid-nineteenth century operated in this way. Yet we are not really told why. What crucial change occurred? Or rather, what was present before the change? The analogy with oxygen is incomplete. While a fire cannot burn without oxygen,

[20] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 2," 563.

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oxygen only rarely and in special circumstances produces a fire. What are these special circumstances?

The root of Haskell's problem, it seems to me, lies in his definition of capitalism. If we move beyond the assumption that capitalism is merely production for the market and for profit, we may be able to understand its relationship with humanitarianism more easily. It is surely preferable to define capitalism as generalized commodity production. Among these commodities is labor power. Capitalism involves not only markets but wage labor, a class of wage-laborers, in fact, selling their labor power. Markets, after all, have existed for millennia, but capitalism, according to this definition, is a more specific occurrence. In order to explain the rise of humanitarianism in general and antislavery in particular, we need to focus on the relationship between capitalist and wage-laborer.[21]

For most of human history, the status of the wage-laborer has been exceptionally low. At least some wage-laborers have been present in most pre-modern societies, and they have been a despised and hated lot. Aristotle believed that to be a wage-laborer was to be virtually a slave, and this attitude seems to have been prevalent in Europe (and perhaps elsewhere) for two thousand years after him. Christopher Hill has shown how the wage-laborer was viewed in seventeenth-century England, and it is apparent that the English, perhaps without knowing it, were good Aristotelians in this respect. It is also easy to show that early American democrats, as late as the Jacksonian era, had much the same attitude. The problem was dependence. Wage-laborers were dependent on those who paid their wages, and the word frequently used to describe the dependence was "slavish." So, when we study the rise of antislavery, it is even more striking that abolitionists should have attacked chattel slavery while defending wage labor. Why the break with the past?[22]

Whereas some historians may be tempted to make light of the growth of wage labor, its impact was actually enormous. The vast increases in material production recorded in the last two centuries have occurred in societies that have been capitalist in this sense. Apart from those that achieved industrialization at the point of the gun, no nation has modernized without wage labor on a large scale. This alone suggests that the relationship between capitalist and laborer is likely to have a profound structural effect on a society. In the nineteenth century, the effects were indeed profound. The home and family ceased to be the center of production they had traditionally been. Similarly, notions of individualism, long potent in England and the United States, came to be modified in many subtle ways. These were some of the processes by which capitalism fostered humanitarianism.[23]

[21] I am, of course, following Marx's definition of capitalism.

[22] G. E. M. de Ste. Croix, *The Class Struggle in the Ancient Greek World* (London, 1983), 179–204; Christopher Hill, "Pottage for Freeborn Englishmen: Attitudes to Wage Labour," in Hill, *Change and Continuity in Seventeenth-Century England* (London, 1974), 219–38.

[23] At one point, Haskell did refer to C. B. Macpherson's notion of possessive individualism, which accords easily with the Marxist definition of capitalism, but he did not follow up the point. Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 2," 553; C. B. Macpherson, *The Political Theory of Possessive Individualism: Hobbes to Locke* (London, 1962).

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I can perhaps best present my argument by comparing abolitionist thinking with that of classical republicanism.[24] Among the key questions for all political and social theorists are these: what is the foundation of individual morality? How is social stability to be achieved? How is loyalty to the state to be secured? The classical republican tradition could not conceive of a stable society unless the vast majority of the citizenry owned their own means of production, either tools and shops if they were to be artisans or, more typically, land if they were farmers. Classical republicans wanted the informed and politically active citizen to be able to place the common good ahead of private and personal gain. The egotistical pursuit of self-interest, it was believed, would plunge a republic into anarchy. Wage-laborers lacked the independence to be able to consider the common good.[25]

Yet wage labor did emerge and on an ever-increasing scale. Not surprisingly, the rise of wage labor called into question many traditional assumptions. In societies that were changing in this way, people altered their attitudes and practices in an attempt to bring them into line with the new realities. They looked for, and found, new supports for individual morality and the social order. Among these were the family, the home, and the individual conscience. None had been previously ignored but all came to be redefined. In societies that had redefined them or were in the process of redefining them, slavery came to appear as a greater and greater evil. Thus a small but growing number of influential individuals were sensitized to the evils of slavery. As they viewed their own society differently, these individuals initiated a new hostility to slavery (and other evils, too) and led the humanitarian movement.[26]

The spread of the market meant that a growing part of human life was subject to the force of individual self-interest. But a society in which the pursuit of self-interest is universal is a society that is about to collapse. The pursuit of self-interest threatens to subvert the rules by which the game is played. Why should self-interested individuals not seek to buy the law? Why should they not sell out the nation for gain? Why should they not rob, kill, or maim their competitors? It might well be in their interest to put an end to the system by which everyone else's interest can be pursued. This, of course, is the paradox of freedom. If all are free, will not some use their freedom to end the freedom of others? A society based on the pursuit of self-interest needs certain institutions, certain practices that must remain outside the area in which self-interest can operate. This requirement perhaps explains why right-wing groups that have sought the free play of market

[24] The interpretation I am offering is in a highly abbreviated form. I am seeking to provide evidence sufficient to demonstrate its plausibility rather than firmly establish the case. Nor have I considered temporal or geographical differences in antislavery opinion. For additional evidence of some of the attitudes I am describing, see Ronald G. Walters, *The Antislavery Appeal: American Abolitionism after 1830* (Baltimore, Md., 1976).

[25] J. G. A. Pocock, *The Machiavellian Moment: Florentine Political Thought and the Atlantic Republican Tradition* (Princeton, N.J., 1975).

[26] It is not necessary to claim that the abolitionists were aware of the social origin of their beliefs. Indeed, the very fervor of abolitionism and of humanitarian reform generally came from the conviction that the ideals espoused were timeless and unconnected with any specific social formation. Although historians should take this belief seriously enough to seek to explain its origin, we need not assume that it is correct.

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forces in the economy have, in a search for transcendental values, often emphasized the nation, the family, the race, and the soil. In order for self-interest to operate, there must be a sphere from which self-interest is barred.

Without ever expressing this notion in these terms, nineteenth-century Americans were fully aware of the need for a sphere in which self-interest did not operate. Behind most of the political controversies of the antebellum period lay an implicit understanding of this most fundamental problem. The tariff controversy concerned the propriety of selling labor power in factories; temperance disputed the right to sell liquor on the market. Slavery raised the question of the sale of human beings. Generally speaking, those who were willing to sell humans were reluctant to see labor power being bought and sold while those most keen to see the spread of wage labor tended to be antislavery.[27] To allow the sale of labor power was to consent to the most dramatic and far-reaching spread of market relations in society and create a need both for an area from which the market would be barred and for a morality to support the new society. The abolitionist argued that the market must be barred from dealing in human beings so that man himself, with his conscience and his "soul immortal," could become the prop for the new social order. Although these new attitudes are clearly expressed in much of the humanitarian writing of the eighteenth and nineteenth centuries, I shall confine myself to American abolitionists in the last thirty years of American slavery.

"THE WHOLE QUESTION OF THE DUTY OF OPPOSITION TO SLAVERY," declared the American and Foreign Anti-Slavery Society, "rests on the sinfulness of reducing innocent men and women, and their children after them, to articles of merchandise." The use of the word "innocent" implies an optimistic view of humanity. More explicitly, the society condemned the practice of converting "into articles of merchandise . . . beings charged with no crime, made a little lower than the angels, and redeemed by His own blood." Abolitionists frequently referred to man's "immortality" and displayed their contempt for those who blasphemously assigned to it a mere commercial value. Thus Gerrit Smith, in an open letter to Henry Clay, referred to "the abhorrent calculation of the worth in dollars and cents of immortal man," and George Bourne asserted that "to traffic in flesh and blood animated by the reasoning capacities is the greatest practical indignity which can be offered to men as immortals."[28] Abolitionists deliberately juxtaposed references to humanity's higher qualities with the language of trade and commerce in order to emphasize their utter incommensurability. This was the strategy that Theodore Dwight Weld employed in order to direct his readers' attention to the enormity of the slaveholder's sin: "ENSLAVING MEN IS REDUCING THEM TO ARTICLES OF PROPERTY—making free agents, chattels—converting *persons* into *things*—sinking

[27] Thus with some exceptions the Democratic party, even in the North, was more tolerant of slavery than the Whig, the party of the tariff and the factory system. See John Ashworth, *"Agrarians" and "Aristocrats": Party Political Ideology in the United States, 1837–1846* (London, 1983).

[28] *An Address to the Anti-Slavery Christians of the United States* (New York, 1852), 3, 2; *Letter of Gerrit Smith to the Hon. Henry Clay* (New York, 1839), 22; [George Bourne], *A Picture of Slavery in the United States of America* (Middletown, Conn., 1834), 35.

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immortality into *merchandize* . . . MEN, bartered, leased, mortgaged, invoiced, shipped in cargoes, stored as goods, taken on executions, and knocked off at a public outcry. Their *rights*, another's convenience, their interests, wares on sale, their happiness a household utensil; their personal inalienable ownership, a serviceable article or a plaything, as best suits the humour of the hour, their deathless nature, conscience, social affections, sympathies, hopes, marketable commodities."[29] Often, abolitionists emphasized that it was above all the human soul that must be placed beyond earthly or worldly considerations. "How can there be room for further wrong," William Alger asked, "when a soul is made a thing?" Eliza Lee Follen told her readers that "he who pretends to own a soul usurps the prerogative of the Almighty," and George Bourne argued that "as no person can possibly offer an equivalent for a human soul, no purchase could ever be honestly made of a rational being." But it was perhaps Harriet Beecher Stowe in *Uncle Tom's Cabin*, surely the most influential antislavery work ever written, who gave fullest vent to the abolitionist's sense of outrage at the attempt to make the human soul a commodity. One of the most powerful passages in the book comes when the narrator describes a slave warehouse in New Orleans. She tells the reader to expect that "you shall be courteously entreated to call and examine, and shall find an abundance of husbands, wives, brothers, sisters, fathers, mothers, and young children, to be 'sold separately, or in lots to suit the convenience of the purchaser'; and that soul immortal, once bought with blood and anguish by the Son of God, when the earth shook, and the rocks rent, and the graves were opened, can be sold, leased, mortgaged, exchanged for groceries or dry goods, to suit the phases of trade, or the fancy of the purchaser."[30] There had to be a rigid separation between those areas of life where the market could rule and those where it was forbidden.

In place of tradition, in place of the stability offered by ownership of one's farm or workshop, the reformer offered the individual conscience. It was an unfailing guide to action. It could not be repressed. Its dictates could not be reasoned away. Referring to a slavetrader, "Uncle" Tom says: "I'm sure I'd rather be sold, ten thousand times over, than to have all that ar poor crittur's got to answer for." Another virtuous character in the novel remarks, "What we are conscience bound to do; we can do no other way." Hence the sinner, as Stowe explains, turned to drink to drown out reflection and the voice of the soul.[31]

Abolitionists generally agreed on the conscience as the underpinning for society. As one of them put it, "our strength all lies in a single force—the conscience of the nation." Even though "all else" was "on the side of the oppressor," this difficulty need not provoke despair, since conscience, "that force of forces when properly instructed, is all, and always, on our side." Wendell Phillips, in the aftermath of John Brown's raid on Harpers Ferry, declared that it was not the old "gray-headed

[29] [Theodore Dwight Weld], *The Bible against Slavery* (New York, 1838), 8.

[30] William Alger, *The Historic Purchase of Freedom, An Oration Delivered in . . . Boston, December 22, 1859 . . .* (Boston, 1859), 8; Eliza Lee Cabot Follen, *To Mothers in the Free States* (n.p., n.d), 2; [Bourne], *Picture of Slavery*, 37; Harriet Beecher Stowe, *Uncle Tom's Cabin: Or, Life among the Lowly* (1852; rpt. edn., London, 1981), chap. 30, p. 467.

[31] Stowe, *Uncle Tom's Cabin*, chap. 7, p. 111; chap. 17, p. 289.

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man" himself that made "Virginia tremble." Instead, Virginia "trembled at a John Brown in every man's conscience." In Theodore Parker's view, conscience was "relatively perfect" and "the last standard of appeal." Gilbert Haven believed that conscience was "employed by our Creator as His representative in the soul."[32] From this premise, it followed that people must obey their consciences. "Let every one settle it as a principle," William Lloyd Garrison exhorted his readers, "that his conscience, and not his lay or spiritual leaders, must be his commander." The conscience must "govern the movements of soul and body." To Parker, it was imperative that, having learned from his conscience "the moral law of God," man must obey it. Indeed "nothing can absolve men from this duty."[33]

These beliefs promoted abolitionism in two ways. First, they made it appear abhorrent to trade in, and assign commercial value to, the human conscience. Second, they ensured that antislavery would be pursued in an uncompromising manner and with all the righteousness of a holy crusade. Northerners were in general opposed to slavery, but those who shared this moral absolutism became its severest critics. The conscience would thus be one barrier against the destructive force of self-interest. A second was the home and the family. The nineteenth-century family as described in the novels of Charles Dickens, for example, was preeminently a refuge, a haven from the tumultuous world outside. This was the abolitionist view, too. Without the family, "the world would be nothing better than one scene of pollution and wo." It would be "a wilderness." The family brought to a man "the comfort and solace of wife and children, whatever may betide him in this rugged world." The family home was "the place where we must cultivate all the narrow virtues which cannot bear the cold atmosphere of the outside world"; it was "a sanctum wherein the world has no right to intrude, where the heart may freely expand in every possible manifestation to which Nature prompts."[34]

Abolitionists believed that the family was an essential counterweight to the spirit of selfish acquisitiveness that an increasingly commercial society promoted. At home, a man "forgets this strife, and all the hardness which the world demands of him, living quietly once more." "The effect of common toil, of intercourse with the business of men, as both are now managed," Parker noted, "tends often to harden the man and make him selfish." Yet "the sweet influence of home" was, fortunately, "just the reverse." "The sphere of a man's daily business" made few demands "on his affections, on the loftier and better sentiments of his nature," since those "he finds not necessary to attain his private ends." Home was the "school

[32] *Revolution the Only Remedy for Slavery* (New York, 1855), 16; Wendell Phillips, *Speeches, Lectures, and Letters*, 1st series (Boston, 1863), 273; Parker, *Slave Power*, 292; Gilbert Haven, *National Sermons, Speeches and Letters on Slavery and Its War . . .* (Boston, 1869), 9.

[33] *Liberator*, 26 January 1833; Henry C. Wright, *Marriage and Parentage: Or, the Reproductive Element in Man, as a Means to His Elevation and Happiness* (Boston, 1855), 249–50; Parker, *Slave Power*, 292–93.

[34] "Letter of Gerrit Smith to Rev. James Smylie," in *Anti-Slavery Examiner*, 1 (1837); *Mr. Allen's Report of a Declaration of Sentiment on Slavery, Dec. 5, 1837* (Worcester, Mass., 1838), 7; [George Allen], *A Report on American Slavery Read to the Worcester Central Association March 2, 1847* (Boston, 1847), 13; Theodore Parker, *Lessons from the World of Matter and the World of Man* (Boston, 1873), 197; Wright, *Marriage and Parentage*, 308.

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for affection and kindly sympathy." It was in the home that "we learn the great lesson of affection, gentleness, tenderness," it was there that "a man learns to trust another, without fear," and in the home, "qualities which our daily calling does not exercise" would experience "a serious and healthful growth."[35]

Abolitionist writers portrayed the family as a potent force for good in the world. The law of conjugal loyalty was "inscribed deep in the nature of man." According to Stephen S. Foster, "the conjugal relation has its foundation deeply laid in man's nature, and its strict observance is essential to his happiness." One anonymous writer declared that marriage was "the most intimate, endearing, and sacred union, that can be formed on earth" so that "a strict and high regard for its sacredness must lie at the foundation of a well ordered and virtuous state of society." Henry C. Wright even went so far as to assert that "in their power over the organization, character and destiny of human beings, the Church is nothing, the State is nothing; religion, government, priests and politicians are nothing, compared to marriage and parentage, to the husband and wife, the father and mother." Wright believed that it was "only in a true home" that "the soul" could "attain its full development in all directions" and there "alone" that "the conscience" could "become a universe of light to guide the soul onward and upward." Theodore Parker reasoned that since "its roots" were "in the primeval instincts of the human race," "the family will last forever."[36]

Abolitionists gave close attention to the relationship between parents and children. They did not simply claim that it was the duty of parents to instill correct moral values into children but expressed a faith in the redemptive powers of children themselves. In an extraordinary passage, Henry Ward Beecher explained the process: "When your own child comes in from the street, and has learned to swear from the bad boys congregated there, it is a very different thing to you from what it was when you heard the profanity of those boys as you passed them. Now it takes hold of you, and makes you feel that you are a stockholder in the public morality. Children make men better citizens. Of what use would an engine be to a ship, if it were lying loose in the hull? It must be fastened to it with bolts and screws, before it can propel the vessel. Now a childless man is just like a loose engine. A man must be bolted and screwed to the community before he can begin to work for its advancement; and there are no such screws and bolts as children."[37] Beecher employed the language of commerce ("a stockholder in the public morality") as well as an extended metaphor that reflected the importance of technology to the age in which he lived. But it was apparent that the process he described depended on the inviolability of the family and required that both home and family be kept strictly separate from the pervasive values of commerce and finance.

[35] Theodore Parker, *Sins and Safeguards of Society* (Boston, n.d.), 209, 211–13;

[36] *Mr. Allen's Report*, 7; Stephen S. Foster, *The Brotherhood of Thieves; Or, a True Picture of the American Church and Clergy* (Concord, Mass., 1886), 11; "A Native of the South-West," *The Family and Slavery*, tract no. 37, American Reform Tract and Book Society (n.p., n.d.), 5; Wright, *Marriage and Parentage*, 276, 291; Parker, *Lessons*, 187.

[37] Beecher quoted in *Atlantic Monthly*, 1 (May 1858): 866.

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In this light, slavery presented a spectacle that was profoundly shocking. "The worst abuse of the system of slavery," according to Harriet Beecher Stowe, "is its outrage upon the family." Since the master could buy and sell slaves at will and since slaves could not legally marry, slavery "breaks into the sanctuary of the home." The power the master had over his female slaves meant that the sanctity of his own family was in constant danger. Gerrit Smith claimed that slavery was "essentially and inevitably at war with the family state." Charles Beecher argued that slavery "nullifies the family" and was, as a consequence, "in direct and flagrant opposition to the law of God." He added that it "must excite his deepest displeasure." Another abolitionist declared that "the Family is the head, the heart, the fountain of society and it has not a privilege that slavery does not nullify, a right that it does not violate, a single facility for improvement it does not counteract, nor a hope that it does not put out in darkness." This writer also pointed out that "those who impose and those who suffer the bondage, alike suffer."[38]

Since the abolitionists had rejected many of the institutions of their nation, they necessarily placed a heavy burden on those that they wished to sustain. The family was preeminent among them. Abolitionists believed that the family was redemptive, a means of supplying the altruism that was threatened by the dissolvent force of self-interest. In *Uncle Tom's Cabin*, the head of one (virtuous) family spells out this function: "'Thee uses thyself only to learn how to love thy neighbor, Ruth,' said Simeon, looking, with a beaming face on Ruth. 'To be sure. Isn't it what we are made for? If I didn't love John and the baby, I should not know how to feel for [Eliza].'" By contrast, a vicious individual like Simon Legree, we are told, rejects both conscience and mother. Indeed, had Legree not rejected his mother, he would have been saved from sin.[39] Even as the family was losing its economic importance, it was being asked to assume this other role. Traditionally, writers had been inclined to view society, or at least the local community, as the family writ large. That is, rather than see the family as a refuge from society, ruled by different conventions and norms, they had often viewed society as a family. The family was society in microcosm. As these attitudes came to be revised, the humanitarian crusades of the eighteenth and nineteenth centuries gathered momentum. The heightened concern for the soul and the individual conscience, nourished within the family circle, necessarily intensified the concern for slavery, which disrupted family ties and offered many temptations and opportunities for the slaveholder to sin. (It is interesting to note that drink was also condemned for undermining the family and for silencing the conscience.) In other words, one route from capitalism to humanitarianism was via an altered understanding of home, family, and conscience. Since it was also possible to claim that slavery slowed the pace of

[38] Harriet Beecher Stowe, *The Key to Uncle Tom's Cabin* (Boston, 1853), 257; "Letter of Gerrit Smith to Smylie," 4; Charles Beecher, *The God of the Bible against Slavery* (New York, 1855), 3; "A Native of the South-West," *Family and Slavery*, 23.

[39] Stowe, *Uncle Tom's Cabin*, chap. 19, p. 331; chap. 13, p. 220. It is tempting to speculate that a shift was occurring at this time in the locus of moral values, a displacement away from the traditional community, in toward the family and the individual conscience and out toward the nation.

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economic advance, it is not surprising that to more people than ever before it seemed an unmitigated evil.[40]

FOR SEVERAL REASONS, AN INTERPRETATION THAT EMPHASIZES WAGE LABOR has more explanatory power than one that focuses on the market. First, as I have indicated, Haskell's view makes a bit of a mystery of the abolitionists' selectivity of concern. On the hypothesis I am proposing, it ceases to be a surprise that the abolitionists accepted wage labor at the same time as they attacked slavery. Indeed, it was precisely because of the spread of wage labor that they attacked slavery in the way they did. Haskell needs to show why a set of attitudes generated by the market made the idea of a market in human beings especially unattractive. The market produced attacks on the market. But to accord causal primacy to wage labor leaves no corresponding difficulty. Second, we are now more easily able to explain a change in values. It is not necessary to claim either that slavery was always regarded as evil (as implied in the starving stranger analogy) or to assume that there is a one-way, causal relationship between changes in recipe knowledge and changes in values. Third, the problem of the South can now be seen not to be a problem at all. A lower level of humanitarian sentiment is precisely what one would expect in a society based on slavery rather than wage labor. Finally, the diverse and contradictory effects of markets in both strengthening and weakening the sense of responsibility cease to present a problem. As Haskell suggested, the market supplied the oxygen. But wage labor was the explosive. While both are necessary, it is an odd explanation of a fire that emphasizes the oxygen rather than the explosive. Indeed, it is the explosive that historians need to handle with care and attention.

[40] It was possible to condemn slavery on either moral or economic grounds, and some individuals pressed one set of criticisms to the exclusion of the other. It was more common, however, for humanitarians to employ the economic arguments, too. A complete explanation of the relationship between capitalism and antislavery must, of course, take account of this. See the works by Howard Temperley cited in note 2.

One group closely involved in wage labor that nevertheless remained indifferent or even hostile to the antislavery crusade was the factory owners of the North who depended for their livelihood on the supply of cotton from the South, a clear case of direct economic interest obstructing the process by which broader economic developments generate ideas.

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AHR Forum

Convention and Hegemonic Interest in the Debate over Antislavery: A Reply to Davis and Ashworth

THOMAS L. HASKELL

I AM DEEPLY INDEBTED TO BOTH DAVID BRION DAVIS AND JOHN ASHWORTH for reading my essay with such care and responding to it so intelligently and creatively. It is rare to have such acute critics and rarer still to have civil ones. My sense of gratitude to Davis is especially strong because of the obvious dependence of my work on his. But for his research into the problem of slavery in Western culture, the question that my article addressed could not have been formulated. Indeed, because my principal concerns have to do with changing concepts of personal agency and moral responsibility, rather than with the history of the movement to abolish slavery, I am in the position of a rude stowaway in Davis's finely crafted historiographical vessel: not only did I come aboard uninvited, I have even challenged the master's authority by proposing a change of course. For the captain to react to this impertinence by accusing me of sins no worse than "misinterpretation," "reductionism," and "positivistic behaviorism" is, I suppose, better treatment than a stowaway has any right to expect.

Cogent though my critics have been, no one will be surprised to hear that there are aspects of my argument that I think they have overlooked or misunderstood, as well as some that my original essay did not develop adequately. Because Ashworth is something of a stowaway himself (and since he seems little more content with the vessel's present heading than I am) I will examine the differences between Davis and myself before turning to Ashworth's proposal for still another change of course.

DAVIS'S RESPONSE TAKES THE FORM OF A ROUSING REAFFIRMATION of faith in the hegemonic mode of explanation, including especially the self-deception argument that both Ashworth and I find unpersuasive. But, even as Davis reaffirms the hegemonic mode, he severely narrows its scope to a single nation, a limited period, and a certain phase of the antislavery movement, its "acceptance" rather than its

A critical reading of an earlier version of this article by Martin Wiener led to important improvements, for which I am most grateful. All responsibility remains mine.

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"origin." The effect of this shift of emphasis is to render the argument less vulnerable to objections but also far less ambitious than I and many other readers have assumed that it was originally meant to be.

In most respects, Davis seems little concerned whether we take his current remarks to be a restatement of his original position, a needed clarification of it, or a revision, a redeployment on more defensible terrain. This is as it should be, for Davis's volumes on slavery constitute one of the towering achievements of historical scholarship in our generation, and the author of a continuing project of this magnitude and high level of distinction need not apologize to anyone for changing his mind. But, on one point, Davis's unconcern gives way to a different tone: in regard to a distinction he is now eager to make between the origins of the antislavery movement and its acceptance, he says that I seriously misrepresented his original views—"here Haskell misquoted me."[1] Davis knows better than anyone else what he intended to say, but I did not misquote him. Nor can I even agree that I was mistaken about the spirit of what he actually wrote. To show that this is true and to clear the way once again for the alternative to the hegemonic argument that I am recommending, I must begin by examining the differences between Davis's original position and the one he now occupies.

The hegemonic argument, Davis now says, "fills only a few pages" of a long book and was meant to apply "only to British history in a limited period from the 1790s to 1823, with some brief speculations reaching ahead to the 1830s. I did not extend the concept of hegemony to America or France, where abolition movements emerged in wholly different contexts. Certainly, I advanced no general theory of abolitionism *per se* as an instrument of hegemonic control (although I would claim it was always related to the need to legitimate free wage labor)."[2] This is a puzzling statement. The last sentence, with its highly elastic parenthetical proviso, is its most problematical part, but the first sentence is also surprising. Far from devoting only a "few pages" of *The Problem of Slavery in the Age of Revolution* to the hegemony argument, Davis organized much of the book around it. In his own words, "much of this book will be concerned with the ideological functions and implications of attacking this symbol of the most extreme subordination, exploitation, and dehumanization, at a time when various enlightened elites were experimenting with internalized moral and cultural controls to establish or preserve their own hegemony."[3] In contrast to his original stress on the hegemonic implications of the movement, Davis now minimizes those implications, in form if not in substance, by drawing a sharp distinction between saying that abolitionism was an instrument of hegemonic control and saying that it was "related to the need to legitimate free wage labor." But what substantive difference stands behind this formal distinction? To say that abolitionism is in some degree explicable in terms of the need to legitimate wage labor is to say that it is, in that same degree, hegemonic. Everything

[1] David Brion Davis, "Reflections on Abolitionism and Ideological Hegemony," *AHR*, 92 (October 1987): 798.

[2] Davis, "Reflections on Abolitionism," 798.

[3] David Brion Davis, *The Problem of Slavery in the Age of Revolution, 1770–1823* (Ithaca, N.Y., 1975), 48, 49.

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therefore depends on what Davis means by "related." If abolitionism is strongly related to the need to legitimate free labor, the parenthetical proviso counts for nothing, and we are right back where we began, with the claim that abolitionism is largely to be understood as an instrument of hegemonic control. If the relation is weak, then abolitionism's hegemonic implications fade in significance, possibly to the vanishing point. Where on this wide spectrum of possibilities does Davis mean to stand?

In the present article, Davis attaches great significance to a distinction between the *origins* of the antislavery movement and its *acceptance* by governing elites. The story of its origins, he says, is the one he told in the first of the three volumes he has published on slavery; it is a tale of the seventeenth and eighteenth centuries, and it concerns authentic religious impulses and radical popular initiatives. None of this, Davis now seems to believe, is much illuminated by the hegemonic argument. Antislavery took on a hegemonic character, according to his current view, only in the 1790s and the early decades of the nineteenth century, when it was appropriated by the aristocratic African Institution, reshaped by wealthy merchant philanthropists, and translated into state policy by conservative governing elites otherwise hostile to social change.[4]

Hegemony was not originally confined to this modest and unobjectionable explanatory role. Consider the following two paragraphs from *The Problem of Slavery in the Age of Revolution*. They present an arresting vision of a dynamic transformation in moral sensibility that defies any neat division into "origin" and "acceptance." Material interests are invoked to illuminate the entire transformation, not a phase of it. It is in connection with these paragraphs that Davis states that he was misquoted.

> The diversities of New World slavery should not blind us to the central point. In the 1760s there was nothing unprecedented about chattel slavery, even the slavery of one ethnic group to another. *What was unprecedented by the 1760s and early 1770s was the emergence of a widespread conviction that New World slavery symbolized all the forces that threatened the true destiny of man. How does one explain this remarkable shift in moral consciousness, if it was not a direct response to an innovation of unparalleled iniquity?* Presumably men of the eighteenth century were no more virtuous than men of earlier times, although something might have altered their perception of virtue. No doubt the new antislavery opinion drew on the misgivings and anxieties which slavery had always engendered, but which had been checked by desire for independence and wealth. Yet the slave systems of the New World, far from being in decay, had never appeared so prosperous, so secure, or so full of promise.
>
> *The emergence of an international antislavery opinion represented a momentous turning point in the evolution of man's moral perception, and thus in man's image of himself. The continuing "evolution" did not spring from transcendent sources: as a historical artifact, it reflected the ideological needs of various groups and classes* [my emphasis].[5]

My summary of these words read as follows: "Like Foucault . . . Davis insisted that the new sensibility 'did not spring from transcendent sources.' Rather, its

[4] Davis, "Reflections on Abolitionism," 798–99.
[5] Davis, *Problem of Slavery in the Age of Revolution*, 41–42.

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origin, he said, lies in 'the ideological needs of various groups and classes.'"[6] The words I had enclosed in quotation marks accurately reproduce Davis's language. It is evidently my use of the word "origin" in place of "continuing 'evolution'" that he objects to, yet in his own text, the latter term presumably refers back to the previous sentence in which he spoke of the "emergence" of international antislavery opinion and the evolution of "man's" moral perception. How different is "origin" from "emergence?" How can "man's" moral perception be understood to refer exclusively to the perceptions of a British elite between 1790 and 1823? Is not a "source" from which attitudes "spring" an origin? Nothing in the original text suggests that Davis thought that the "origin" of antislavery opinion and its "acceptance" (or "continuing evolution") were distinct chronological phases centered on the 1790s or that they posed crucially different problems of explanation. Davis described, in the two paragraphs quoted above and in the adjacent pages, the emergence in many minds of an unprecedented conviction that slavery constituted an intolerable evil. That conviction, he specifically said, was already "widespread" by the 1760s. He called it a "momentous turning point in the evolution of man's moral perception" and a "remarkable shift in moral consciousness," and he took pains to insist that it did not spring from transcendent sources.

He originally invoked "ideological needs" to explain not just the "acceptance" of the movement by conservative governing elites after the 1790s but the entire process by which a deep repugnance for the principle of slavery spread beyond the handful of sectarians and eccentrics who first expressed it and gradually gained strength in ever wider and more influential circles. It was, in Davis's view, ideological needs that prepared a receptive audience, and this effect was being felt before the nineteenth century. "By the eighteenth century," Davis wrote, " . . . profound social changes, particularly those connected with the rise of new classes and new economic interests in Britain and America, created an audience hospitable to antislavery ideology." Although he was always extremely careful to avoid any simple reduction of antislavery opinion to class interest, Davis clearly believed that material interest affected not only the political repercussions of the new sensibility but also the emergence of the sensibility itself: in his own words, "material considerations . . . helped *both* to shape the new moral consciousness and to define its historical effects" (my emphasis).[7]

Any attempt to exempt religion from the hegemonic mode of explanation, or to confine hegemonic considerations to the period after the 1790s or to governing elites, would make a shambles of the interpretation Davis presented in the pivotal chapter of his book, "The Quaker Ethic and the Antislavery International." In that chapter, he ranged brilliantly across the entire last half of the eighteenth century and traced a multitude of interconnections between Quaker organizational skills, religious principles, and concerns about labor discipline. The fabric he wove displays no seam between "origins" and "acceptance." Eighteenth-century Quak-

[6] Thomas L. Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 1," *AHR*, 90 (April 1985): 344.

[7] Davis, *Problem of Slavery in the Age of Revolution*, 82, 48.

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ers in both England and America, not a nineteenth-century British governing elite, first demonstrated that "testimony against slavery could be a social correlative of inner purity which seemed to pose no threat to the social order—at least to that capitalist order in which the Quakers had won so enviable a 'stake.'" It was also the transatlantic community of Quakers about which Davis was speaking when he formulated the keystone of his entire interpretation, the idea that "as a social force, antislavery was a highly selective response to labor exploitation" and one that "gave a certain moral insulation to economic activities less visibly dependent on human suffering and injustice."[8] If this is not the hegemonic argument, I am at a loss to know what would be.

Ambiguous though his present position is, Davis has obviously abandoned much of the territory he once claimed under the banner of hegemony. In spite of his rousing reaffirmation, a considerable disenchantment with the hegemonic mode of explanation is plainly evident in his attempts to confine it (formally, at least) to a narrower range of events. Although I am a critic of the hegemony argument and believe that some sort of retreat from exposed terrain was in order, I am not convinced that this particular move was the right one to make. The sponsors of the African Institution certainly make a more likely target for hegemonic explanation than do the Quakers, and living in Reagan's America gives us ample demonstration that clamorous attacks on distant evils can function effectively to obscure problems uncomfortably close at hand. But, by confining hegemonic considerations so narrowly, Davis threatens to tear apart the rich and subtle network of connections between consciousness and society that make *The Problem of Slavery in the Age of Revolution* a historiographical landmark.

Discovering non-reductionist ways of relating consciousness to social structure and change is, in my view, the most pressing historiographical issue before us today. And, for all its many drawbacks, the hegemony schema—self-deception and all—did enable Davis to bring the economic developments that we associate with the rise of capitalism into extremely close explanatory conjunction with the new humanitarian sensibility (especially in the lives of the Quakers) without falling victim to either of the excesses that usually spoil efforts to relate conciousness to social structure: he neither disregarded the stated intentions of the reformers nor projected into their activities any implication of conspiracy. So successfully did he navigate the treacherous divide between consciousness and society, in fact, that in spite of my reservations about hegemony, if forced to choose between Davis's original position and the present one, I might well prefer the former. It at least acknowledged the existence of pervasive links between consciousness and society in all phases of the antislavery movement and did not imply that authentic moral innovators—the "originators" of antislavery as opposed to those who merely "accepted" it—must be seen as uncaused causes. I would rather cling to the frail reed of hegemony than accept the claim that shifts of moral sensibility spring from transcendent causes, beyond the power of any social or economic analysis to

[8] Davis, *Problem of Slavery in the Age of Revolution*, 251.

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illuminate, and then become tainted by material concerns only when they spread to the upper classes.

This is not to say, however, that I find the hegemony model adequate. It grapples with the problem of relating consciousness to society but does not carry us very far toward a satisfying formulation. In logic, if not in fact, its principal appeal ought to be to people who once construed the relation between consciousness and society in classic Marxian terms (and thus were confronted with an anomaly in the failure of conflicting class interests to generate a rising curve of working-class radicalism) but who have come to regard the classic formulation as excessively reductionistic and need an alternative that does not carry them beyond the Marxian pale altogether. Because the Gramscian alchemy acknowledges the existence of pluralism and consensus, even as it transmutes them into proof of domination, it serves—paraphrasing what Erasmus Darwin said about the relation of Unitarianism to Christianity—as a feather pillow, perfect for catching falling Marxists.[9] The fact that people other than falling Marxists, including myself, occasionally find it comforting is attributable less to its intrinsic merits than to the dearth of alternative ways of formulating the relationship between consciousness and society.

The difficulties of the hegemonic mode of explanation are notorious and need no extensive rehearsal here. Its most sophisticated defenders strive to make it more flexible, allowing that there can be "open" as well as "closed" hegemonies and that hegemony need not imply control from the top down but can "bubble up from below." When Jackson Lears writes that "even the most successful hegemonic culture creates a situation in which the dominant mode of discourse—and each visual or verbal text within it—becomes a field of contention where many-sided struggles over meaning are constantly fought out," he succeeds in accommodating the schema to the diversity and complexity of modern society but only at the expense of tacitly acknowledging that hegemonic and pluralistic societies are almost impossible to tell apart.[10]

This mode of explanation is already much too flexible. There seems to be no concrete situation around which it cannot be wrapped. No doubt the upper classes exert a disproportionate influence in cultural matters, but it is specific instances that we need to be able to identify as hegemonic or not, and the question of whether a particular social consensus is "spontaneous" or the product of ruling-class influence, conscious or otherwise, is scarcely ever open to empirical resolution. Even to hope for empirical testability is thought a gauche display of positivism in some circles, but many scholars, including some who have impeccable antipositivist credentials and are eager to see the Gramscian schema work, have acknowledged that the problem is a grave one.[11]

[9] Gertrude Himmelfarb, *Darwin and the Darwinian Revolution* (New York, 1968), 15.

[10] T. J. Jackson Lears, "The Concept of Cultural Hegemony: Problems and Possibilities," *AHR*, 90 (June 1985): 587, 591.

[11] Lears, "Concept of Cultural Hegemony," 579; Robert W. Westbrook, "Goodbye to All That: Aileen Kraditor and Radical History," *Radical History Review*, 28–30 (1984): 69–89; E. P. Thompson, *The Poverty of Theory* (New York, 1978), 37–50.

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To adopt Davis's practice, which identifies as hegemonic any measure that reinforces ruling-class standards or otherwise stabilizes a regime, would be to make all societies always hegemonic, for any measure short of handing over the reins of power to a new ruling class can—by virtue of the more extreme concessions it forestalls—be said to stabilize and reinforce the status quo.[12] It seems unlikely that rulers ever knowingly destabilize their regimes except for the sake of what they perceive as a higher stability, and even the most dramatic concessions to radical critics can always be construed as co-optation from a standpoint still more radical. Hegemony, by this standard, becomes indistinguishable from governance. I would not claim that this unwanted flexibility renders the concept entirely unusable for explanation, but it does make it highly volatile. Like dry ice, hegemony always tends toward sublimation, becoming merely a diffuse aspect of the human condition rather than a distinct feature of particular societies that one could ever point to in explanation of specific events and actions.

Davis's reply provides a striking illustration of the tendency of arguments in the hegemonic mode to float off in directions that have little to do with historical explanation. He gives as an example of hegemony—indeed, an illustration of "precisely what is meant by 'ideological hegemony'"—the "reinforcement of ruling-class standards" that occurred when radicals were "forced" by the dichotomous terms of the antislavery debate to argue that "in some fundamental respects wage earners were no freer or better off than were slaves." After all, he observes, "there could be no lower standard."[13] Problems abound in this statement. To begin with, I doubt that "forced" is the right word here, either for the degree of restraint felt by radicals or the degree of control over discourse exercised by the rulers. Also, there is room to doubt that people who liken their condition to that of slaves thereby accept a low standard—on the contrary, by rhetorically equating one's own possibly bold demands with the slave's humble plea for liberty, one may be setting a very high standard. Consider the hyperbolic efforts by Thomas Jefferson and other patriots on the eve of the Revolution to characterize British policy as a "deliberate, systematical plan of reducing us to slavery"; surely, this rhetoric did not imply the acceptance of a low standard and neither was it evidence of George III's secure hegemony.[14]

It is in his next paragraph that Davis leaves behind historical explanation altogether and enters a dimension of abstract meanings that is quite detached from any judgment scholars might want to make about how things happened or who was responsible for them. After conceding that abolitionism served in some instances to stimulate radical protest against other forms of oppression, Davis argues that the movement also produced effects that worked in the opposite direction: "analogies with chattel slavery may also have retarded the development

[12] Davis, "Reflections on Abolitionism," 809, 808. It should also be noted that if, as Davis now says, "reinforcement of ruling-class standards is precisely what is meant by 'ideological hegemony,'" it becomes very difficult to see how hegemonic considerations can be confined to one country or period. This raises the question of just how far Davis means to go in abandoning hegemony.

[13] Davis, "Reflections on Abolitionism," 809.

[14] Jefferson quoted in Gordon S. Wood, *The Creation of the American Republic, 1776–1787* (Chapel Hill, N.C., 1969), 39.

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of a vocabulary that could depict more subtle forms of coercion, oppression, and class rule."[15] Obviously, if anything had the retarding effect Davis describes, it was slavery itself, not the attack on it. If the reformers had withheld their attack, prolonging the life of the institution, a vocabulary suited to more subtle forms of oppression would presumably have been even slower to develop. So Davis cannot mean that in the absence of antislavery rhetoric a refined vocabulary would have developed earlier. Instead, his point, valid and unobjectionable in itself, is simply that as long as people were preoccupied by the stark oppressiveness of chattel slavery, milder forms of oppression seemed minor by contrast, and this condition, which was utterly beyond the control of the reformers, helped discourage other kinds of challenges to the status quo. This tells us nothing about why people *attacked slavery, why the movement grew, or even how abolition per se contributed* to hegemony; Davis is not in this passage trying to give a causal explanation of anything, he is merely savoring an irony, a "moral ambiguity" that inescapably attended the existence and overthrow of slavery. Such ambiguities are well worth noting as long as they are not mistaken for explanatory assertions. The danger is that some readers (all but the most careful, I suspect) are likely to come away with the preposterous impression that this supposed effect is one more of the hegemonic consequences that abolitionists unconsciously intended—that antislavery flourished partly *because* its rhetoric retarded the development of a more extensive vocabulary of oppression.

But the frailties of the hegemonic mode and the degree of Davis's ambivalence about it are of only passing interest. At the heart of the disagreement between Davis and myself is a different way of formulating the relationship of consciousness and society. That there is a relationship—that consciousness is shaped to a great extent by the social situation of the thinker—both of us take for granted. Davis construes the relationship mainly in terms of class interest and relies on self-deception to introduce a saving element of indeterminacy into the linkage between humane intentions and hegemonic consequences. My formulation, while acknowledging the reality of self-deception and not denying that interest plays an utterly indispensable role in the explanation of human affairs, directs attention instead to another factor that is generally ignored: the role played by social conventions, especially those governing causal attribution and thereby establishing the outer limits of moral responsibility. The stress on conventions and skepticism about the explanatory value of self-deception are the two aspects of my thesis that Davis finds most objectionable. Indeed, apart from his claim that I exaggerated his reliance on the hegemonic argument and his empirical complaints that my thesis cannot account for the existence of women abolitionists or the weakness of antislavery in Holland (points to which I will return), his reply consists largely of an impassioned reaffirmation of the reality of self-deception, coupled with warnings about the moral dangers of convention as an analytical category.

Although Davis misunderstands my views on self-deception, he is correct that we are at loggerheads over the issue of convention. I believe that the unprece-

[15] Davis, "Reflections on Abolitionism," 809.

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dented surge of humanitarian activity in the century following 1750 can be traced to a shift in the network of conventions that govern the attribution of moral responsibility and personal agency. In my view, such conventions exist in all societies, and they are no cause for dismay: without them, no one could possibly know how to apply abstract moral prescriptions such as the Golden Rule in concrete cases. From Davis's perspective, convention and morality tend to be mutually exclusive, and all talk of convention merely shrouds in an unwholesome neutrality the evils associated with "biased consciousness"—self-deception, selective attention, rationalization, mechanisms of avoidance, displacement, projection, and so on. He seems sincerely to believe that my preoccupation with convention can only shelter evildoers from the blame they deserve and sap our will to resist palpable evils such as the Holocaust and the growing threat of nuclear annihilation.[16] These fears are misdirected.

I HAVE NEVER DOUBTED EITHER THE REALITY OF SELF-DECEPTION or its ubiquity. "The problem with self-deception," I originally wrote, "is not that it is a rare mental state or overly technical term. All of us can recall episodes in our lives when we ignored or denied what now seems the plain and reprehensible meaning of our actions—moments when, to paraphrase what Sigmund Freud said about dreams, we knew what the consequences of our action would be but did not know that we knew."[17] I argued not that self-deception does not exist but that it must be distinguished from other deceptively similar states of mind. Although the *unconsciously intended* consequences that self-deception implies are very different in moral tone and explanatory significance from the *unintended* consequences that make up much of what happens in human affairs, the two are not readily distinguishable empirically. Because every event or condition generates a potential infinity of unintended consequences (think of Cleopatra's nose or the two-penny nail for want of which the battle was lost), it is very difficult to show that a specific consequence belongs not in this immense category but rather in the special and much smaller category of consequences that are unconsciously intended. I said that this difficulty "limited" the utility of the concept for historians, not, in Davis's words, that it "invalidates any historical explanation based on the concept of self-deception."[18]

Davis and I do not disagree about the reality of self-deception but about the sort of warrant a historian needs in order to establish the existence of self-deception in particular cases. Granting, as I always have, that one of the many and often contradictory effects of the attack on slavery was to cast the wage-labor system in a comparatively favorable light, I do not find either in Davis's book or in his reply to my previous article any adequate warrant for his claim that this effect was

[16] Davis, "Reflections on Abolitionism," 802.
[17] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 1," 348–49.
[18] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 1," 349; Davis, "Reflections on Abolitionism," 801.

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unconsciously intended by substantial numbers of reformers.[19] Davis still contends that it was and offers as example the case of abolitionist James Stephen, whose blend of sincere humanitarianism and chauvinistic patriotism does not seem to me to cast any light on the question.[20]

Davis also cites as an "obvious example" of self-deception people who respond favorably to advertisements for "lite" beer and cigarettes. "Because the actual effects of smoking cigarettes and consuming alcoholic beverages are well known to the public, the success of the advertisements cannot be attributed simply to ignorance or gullibility," he writes.[21] No doubt self-deception accounts for some of the sales of these products, but would we really want to say that it accounted for all or most of their success? Surely, the ignorance and gullibility that Davis mentions explain a large share, and different attitudes toward prudence and risk aversion—which he does not mention—explain still more. The health risks of smoking pale beside those associated with skydiving, spelunking, rock-climbing and automobile racing, yet these activities do not necessarily entail self-deception. Even daredevils need not deceive themselves; they need only define interest and acceptable risk in eccentric ways. Much less does it require self-deception to smoke cigarettes or drink "lite" beer. Human perceptions vary: some people prize forethought, others value spontaneity; some get their kicks hang gliding, others refuse to fly even in airplanes. Only at the far end of this spectrum, as we approach behavior that is plainly suicidal, do we find a boundary between realistic and unrealistic perceptions of risk that is independent of the perspective of the observer. Only there, it seems to me, can we confidently say that risk-takers deceive themselves. Human interest is less a matter of objective fact and more a matter of subjective interpretation than Davis allows.

A similar overestimation of the accessibility of objective knowledge is built into much of what Davis says about self-deception. For example, in summing up his discussion of the subject, he says that we can justifiably speak of self-deception whenever a group of historical actors "believed they were advancing the interests of all humanity when they were actually promoting the interests of a special class."[22] This is not a workable test of self-deception. It ignores the problem of distinguishing between consequences that are brought into being by the actors' unconscious intentions and consequences that are wholly unintended but that

[19] Davis suggests in his reply that his principal concern is not with individual motivation but with "social meanings." This is very hard to reconcile with his ardent defense of the analytical value of the category of self-deception. Self-deception is a vital element of his interpretation precisely because of what it says about the unconscious motivation of individuals. Moreover, meaning resides nowhere but in individual minds: we can speak of meaning as perceived by actors, as perceived by their various contemporaries, and as perceived by us, looking back retrospectively on events—all these discriminations are well worth making, and perhaps these are what Davis has in mind when he speaks of "social meanings." But there is no meaning that resides in society itself. See Davis, "Reflections on Abolitionism," 799, 802.

[20] Davis, "Reflections on Abolitionism," 801. A much harsher side of Stephen emerges in his plans to bring West Indian freedmen under the sway of "the rational predicament"—work or starve—but the harshness of the plan does not in itself tell us anything about unconscious motives. See David Brion Davis, *Slavery and Human Progress* (New York, 1984), 218–19.

[21] Davis, "Reflections on Abolitionism," 801.

[22] Davis, "Reflections on Abolitionism," 802.

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happen, in retrospect, to serve the interests of the actors (or fit some interpretation of those interests). The word "actually" also implies in this context the existence of a perspectiveless knowledge that we rarely possess in moral matters. Morally interesting acts ordinarily are susceptible to more than one empirically accurate description, and their moral quality varies with the description given. Davis's statement tacitly assumes that all morally competent people perceive the same objective good and therefore arrive at the same description—unless they deceive themselves. To those of us who are less optimistic than Davis about the availability of objective knowledge, it is virtually a defining feature of the human condition that there is no course of action so pure that its disinterestedness and universality cannot be challenged from some plausible point of view—after all, if liberating slaves does not qualify as disinterested, what would? If no course of action is proof against the charges of interestedness, then, by Davis's test, all moral choices become self-deceptive in some degree or from some point of view. Defined this loosely, the category possesses little discriminatory power.

Davis employs an even looser test of self-deception when he speaks of the sixteenth-century Jesuits in Brazil, who strove to protect Indians from enslavement and mistreatment even while encouraging the slave trade. The historian of the Jesuits, Serafim Leite, argued that, although this choice seems oddly "selective" from our standpoint, it would be wrong to call it unjust, just as it would be wrong to complain that a person who founded a hospital for tuberculosis thereby committed an injustice against the victims of leprosy. Davis disclaims any interest in mere "blame," as opposed to the "moral ambiguities of history," but he makes much of the fact that Leite chose to mention leprosy, a disease historically associated with white prejudice against blacks, and he clearly feels that Leite's judgment on this issue is morally flawed. Presumably thinking in terms of self-deception rather than conscious bias, Davis concludes ominously that Leite "accepts" a traditional distinction that prejudicially singles out blacks as suitable for enslavement.[23]

To see how loose a standard of self-deception Davis is applying here (both to Leite and to the Jesuits), we need only turn the tables. How would we respond if a historian who had devoted his career to the history of slavery (David Davis himself, let us say) was accused on that account of unjustly and prejudicially neglecting the history of the whites' oppression of Indians? Historians' career choices, too, are selective, and certainly one predictable consequence of devoting one's career to the history of slavery is to pass up the opportunity of drawing public attention to the plight of Native Americans.[24] Unconscious intention or prejudice could account for such a choice, and no doubt a speaker for Native American interests, justifiably eager to attract attention to the cause, might feel some temptation to adopt such an interpretation, at least for polemical purposes. "If you

[23] Davis, "Reflections on Abolitionism," 804.

[24] The list of opportunities omitted stretches potentially to infinity, as does the stream of unintended consequences that may be said to stem from any act or omission; so the historian in question might equally well be accused of neglecting the Holocaust, nuclear annihilation, environmental catastrophe, and so on.

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are not for us, you are against us" is a familiar refrain among activists competing for public support. But it is a fallacy, and, in the absence of other evidence tending to show prejudice, we would, I think, decide that it was not only invalid but frivolous to take the historian's choice of slavery as a sign of prejudice toward Native Americans. The world is overflowing with suffering strangers; choosing to help one does not signify an intention to prolong the suffering of the others. It plainly will not do to claim that people must intend, either consciously or unconsciously, all the consequences that predictably follow from their choices. Some predictable consequences are intended, some are not; the difference, though by no means entirely arbitrary or irrational, is inescapably a matter of convention and cannot be ascertained without close attention to the norms of the relevant community—without attending, in other words, to the range of meanings that a certain culture makes available to its members.

Davis's strongest evidence for thinking that the abolitionists deceived themselves about the hegemonic consequences of their attack on slavery is that some of their contemporaries accused them of exaggerating the suffering of distant slaves while neglecting that of nearby wage-laborers. Davis assumes that, even if the reformers were initially unaware that one consequence of their attack on slavery would be to cast the wage-labor system in a comparatively favorable light, these contemporary critics—proslavery writers on the one hand and radical labor leaders on the other—brought it to their attention. Once they had been made aware of it, Davis assumes that it became part of their intention, unless they specifically disavowed it. In his present article, Davis concedes that, in the face of this criticism, an abolitionist who was genuinely opposed to all forms of oppression might nonetheless have chosen to give priority to the struggle against slavery on the grounds that it was the most "flagrant and remediable" form of oppression, but Davis assumes that "such a course would entail a disavowal of the claims of proslavery writers and at least a private expression of regret over the unintended consequence of extolling free wage labor."[25] Finding in the archives no expressions of regret and taking into account the rapturous extremes to which abolitionists sometimes went in contrasting free labor with slave labor, Davis concludes that the hegemonic implications of antislavery were indeed intended, though only unconsciously.

Why should we find Davis's imputation of hostile (though unconscious) intention any more persuasive than that of the hypothetical Native American advocate above, who sought to advance a worthy cause by accusing anyone who did not embrace it of deliberately opposing it? Is that not what labor leaders like William Cobbett were doing when they lambasted the abolitionists as "the hypocritical sect of negro-loving philanthropists"?[26] Or when they complained, as the *Poor Man's Guardian* reported in 1833, that the slave worked "only 55 hours and had everything provided for his comfort" while "the mechanic of England

[25] Davis, "Reflections on Abolitionism," 800.

[26] Cobbett quoted by Seymour Drescher, *Capitalism and Antislavery: British Mobilization in Comparative Perspective* (New York, 1987), 252 n.41.

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procured a bare subsistence by 84 hours hard work"?[27] Was anything weightier than this at stake in the accusations of selectivity hurled by the defenders of slavery? Proslavery writers certainly needed no deep insight into the fundamental continuities between free and coerced modes of labor discipline to see that it was to their advantage to accuse their antagonists of hypocrisy and to set the various components of metropolitan opinion against one another by complaining that the self-appointed protectors of the Caribbean slave cared nothing for the suffering of their neighbors. English labor leaders could not fail to see that one way of hitching their own cause to the immensely popular and remarkably successful campaign for abolition was to equate the misery of free workers with that of slaves and to pound away on the selectivity of the reformers.[28] "If you are not for us, you are against us" is a fallacy, but it is a useful one for activists competing for scarce resources.

By accepting the claims of the abolitionists' critics at face value, Davis construes as a dispassionate and objectively true report what may more fruitfully be understood as a familiar rhetorical tactic to change behavior. It is certainly true that when we disapprove of people's behavior and wish to alter it, we often can do so by disregarding their actual intentions and treating them as if the behavior in question could only signify some sort of reprehensible intention. Thus inattentive spouses are routinely accused of spurning their mates; whether the imputed intention is accurate or not is irrelevant to its tactical efficacy. The leverage this tactic is capable of producing one-to-one can be immensely magnified when large numbers of people threaten the same deliberate misinterpretation. This is what happens, for instance, when university trustees are put on notice that the small measure of stability that they have unintentionally supplied to an oppressive regime (South Africa or Russia) by investing in stocks of companies doing business there will henceforth be interpreted as an intended endorsement of that regime and all its worst practices. Deliberately misinterpreting another person's intentions is a shabby way of treating them, but this process of "creative misinterpretation," as it might be called, is no trivial charade, and any movement or political persuasion that abstained from the use of this manipulative strategy would be paying a high price for purity. Cultures can be transformed in this way, and, once the network of conventional meanings available in a culture has been rewoven, by this or other means, the rules for imputing intentionality will have truly been changed, so that what was once an unintended consequence or meaning will conventionally be construed as intentional.

But this change is not accomplished overnight—or in a year or a decade. Culture always remains an arena of contested meanings. More "misinterpretive" projects are always underway than can succeed, and few bring about the permanent transformation they aim at. Until such a project achieves a very substantial degree of success, the instrumentally skewed accounts its supporters give of their opponents' motivation remain just that—skewed accounts or misinterpretations—

[27] Drescher, *Capitalism and Antislavery*, 149–50.
[28] The wide popularity of the movement has been stressed by Drescher, *Capitalism and Antislavery*.

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no matter how good the cause. To mistake them all for innocent reports of the truth would inundate historians with a flood of seemingly malicious intentions and render us incapable of understanding what is going on.

Davis's evidence for thinking that the abolitionists unconsciously intended to legitimate free labor by confining their attack to slavery consists essentially of two classes of facts: the first showing that critics advanced this interpretation (or something close enough to it to lend it credibility), and the second that the abolitionists did not take the trouble to disavow in writing the intention to legitimate free labor. Would we accept similar evidence as a basis for judgments about the unconscious intentions of political activists in our own day? Would we agree, for example, that twentieth-century liberals are guilty of an unconscious desire to foster "welfare dependency"—unless they publicly disavow it? If we accept Davis's evidence, it would seem that we should, for dependency is one predictable consequence of welfare programs, and conservatives have not been shy about alerting the public to it. Of course, conservatives, by the same measure, become guilty not only of indifference but of harboring a deliberate intention to inflict hunger and malnutrition on the poor, for these are predictable consequences of cutting back on welfare. Likewise, by Davis's criteria, feminists favoring abortion rights would stand convicted of unconsciously intending the destruction of thousands of fetuses a year, and right-to-life advocates of wanting to repress women and confine them to traditional roles. College trustees who oppose divestment would be guilty of endorsing apartheid and those who march in protest against them of seeking "easy grace." All of us who read and write about slavery, instead of marching against nuclear weapons, would manifest a latent death wish. The list could go on and on, but the drift toward absurdity is clear enough. If we demand no more evidence than Davis provides, no person or group is free of self-deception and malicious intention. Not that all of these imputations of unconscious intention are flatly false; some may be accurate accounts of the motivation of some individuals. Nor am I saying that historians should never adopt the motivational interpretations set forth by one party to a dispute. Instead, if we want the idea of self-deception to carry any analytical weight, historians need stricter criteria. We cannot make sense of human affairs if we treat all predictable consequences of human choice as intentional. Many predictable consequences are neither consciously nor unconsciously intended, and, since culture cannot be transformed overnight, many remain unintentional even when a vocal interest group is hard at work to intentionalize them.

I am wary of self-deception for much the same reason Davis is wary of convention: each of us fears that the other's approach is self-indulgent. Indignation is a powerful appetite, and nothing satisfies it so abundantly as the idea that our opponents are deceiving themselves—that, in their heart of hearts, they perceive moral issues just as we do, and know that we are right to condemn them. By imputing self-deception to those we criticize, we affirm the universality of the standards by which we judge them and set aside the disturbing possibility that the world is one in which equally competent moral reasoners can arrive at conflicting interpretations of right and wrong.

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Davis's argument presupposes that conscience dictated a single path of moral duty in the era of abolition: opposition to all forms of labor exploitation, whether slave or free. Observing that most abolitionists confined their protest to chattel slavery, Davis wonders (in spite of his admission that slavery was the most "flagrant and remediable" form of exploitation) why they strayed from the path of objective duty. Self-deception is his answer. The question is made necessary by the presupposition that morality is in the fullest sense objective; once we give up the idea that conscience has a single message, we will no longer be puzzled by the abolitionists' failure to take on a larger task than they did, and we will feel little incentive to resort to the idea of self-deception. I will have more to say about selectivity at the end of this article, when I respond to Ashworth's criticism, but for now it is enough to observe that Davis's pivotal question—why were the abolitionists selective in their response to labor exploitation?—is prompted in large part by his apparent conviction that morality is a matter of objective fact rather than interpretation.

WHEN DAVIS WROTE *The Problem of Slavery in the Age of Revolution*, he was more appreciative of the importance of convention than he seems to be now. In spite of the prominence he assigned to self-deception, much of what he originally wrote implied, logically, not that the reformers deceived themselves but rather that their vision was confined within certain conventional limits. For example, when Davis spoke of the reformers furthering the interests of their class only "unwittingly" and said that it would have been "unthinkable" and "inconceivable" for them to recognize the hegemonic implications of antislavery, he gave us a glimpse of a kind of explanation that would not depend on self-deception or unconscious intention and would not necessarily point toward hegemonic interests.[29] "Unthinkability" does not denote self-deception but a meaning that is genuinely unavailable to the actor and is therefore not a part of intention, conscious or otherwise. Similarly, Davis's remark that "prior to the Revolution, few colonists were capable of the imaginative leap of placing themselves in their slave's position" and his observation that "neither Luther nor Calvin . . . had any notion that Christian liberty could alter the fact that some men are born free and others slaves" also acknowledge in convention a constraining power over moral judgment that Davis now seems to find worrisome.[30]

The question of whether morality can be authoritative when it is admitted to have no firmer or deeper base than social convention is, of course, a problem of immense philosophical importance. Neither Davis nor I mean to set up shop as philosophers, but in this regard we unavoidably take well-worn positions in an ancient controversy. I am not sure to what extent Davis would be prepared to treat moral obligation as an objective fact, inherent in the very nature of mankind and the world, timeless in character and therefore not dependent on human consciousness or social convention. But he is certainly closer to that honorable

[29] Davis, *Problem of Slavery in the Age of Revolution*, 253, 350.
[30] Davis, *Problem of Slavery in the Age of Revolution*, 279, 44.

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tradition of moral philosophy, often identified with Plato and Aristotle, than I am. Although I am not at all in sympathy with the reckless, Derridean assault on "foundationalism" that is fashionable in some literary and philosophical circles, I am enough of a historicist, in the contemporary sense of that term, to believe that human morality is neither natural nor divine but a historical product, and it therefore has an inescapably conventional component.[31]

Temperamentally, Davis and I are not far apart. I lament, as I believe he does, the evasiveness of our current "emotivist" or "therapeutic" culture, in which the word "moralist" has become an epithet, and all talk about moral obligation is liable to be taken as a mask for personal preference. We agree, I believe, that a culture that acknowledges no difference between the statements "I *ought* to do this," and "I *want* to do this"—the former an invocation of objective obligation, the latter a report of merely subjective desire—is in deep trouble.[32] But there is, in my view, no help to be had outside the sphere of history and convention. Notwithstanding the persistent and sometimes eloquent protests of Leo Strauss and others, it is widely acknowledged among philosophers that three centuries of inquiry into the basis of moral judgment have shown that ultimate foundations are not to be found, in nature or anywhere else. Instead, our best hope is to demonstrate that treating morality as conventional rather than natural does not mean we are setting it adrift and leaving it at the mercy of every ripple of fashion and stray breeze of personal whimsy. Convention need not be the same as fashion, and the place of convention in moral judgment can be acknowledged without capitulating to subjectivism. Morality can, I believe, possess a constraining and even objective (at least, non-subjective) quality, even though it is conventional.[33] In one of the most widely influential books of our time, Thomas Kuhn has demonstrated that, when scholars give up the idea that scientific thinking aims at correspondence with an objective reality, timeless and independent of human consciousness, the authority of scientific knowledge is not diminished, the familiar practices of rational debate and justification among scientists are not discredited, and science remains as real, as rational, and as relevant to the conduct of life as ever.[34] I am confident that much the same can be said about moral thinking when we give up the contention that it aims at correspondence with truths that are immune to time and have origins somewhere outside human society.[35]

[31] A list of books relevant to this wide-ranging debate that began in literary studies a decade or more ago, and about which historians cannot much longer remain innocent, would be impossibly long. Some exceptionally lucid works are: Jonathan Culler, *On Deconstruction* (Ithaca, N.Y., 1982); Allen Megill, *Prophets of Extremity: Nietzsche, Heidegger, Foucault, Derrida* (Berkeley, Calif., 1985); Gerald Graff, *Literature against Itself: Literary Ideas in Modern Society* (Chicago, 1979); Richard Rorty, *Philosophy and the Mirror of Nature* (Princeton, N.J., 1979); Frederick Crews, "The House of Grand Theory," *New York Review of Books*, 33 (29 May 1986): 36–42; and David Hollinger, "The Knower and the Artificer," *American Quarterly*, forthcoming.

[32] Alasdair MacIntyre, *After Virtue: A Study in Moral Theory*, 2d edn. (Notre Dame, Ind., 1984); Philip Reiff, *The Triumph of the Therapeutic: The Uses of Faith after Freud* (New York, 1966).

[33] I have argued this point at greater length in "The Curious Persistence of Rights Talk in the 'Age of Interpretation,'" *Journal of American History*, forthcoming.

[34] Thomas A. Kuhn, *The Structure of Scientific Revolutions*, 2d edn. (Chicago, 1970).

[35] The preeminent work in recent moral philosophy is that of John Rawls, who specifically rejects "rational intuitionism" and identifies his own position in conventionalist terms: "What justifies a

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To demonstrate the large and vital role that convention necessarily plays in our moral thinking, my original article set forth a hypothetical exercise, the "case of the starving stranger," to which Davis and Ashworth give little attention.[36] This exercise embodies the crucial anatomical features of the process that I believe gave rise to the humanitarian sensibility, and anyone wishing to challenge my thesis will find their target here. In particular, I should think that anyone who is disturbed, as Davis is, by the large role I assign to convention, would want to grapple with this exercise and try to explain what it is, if not convention, that allows us to feel that we have lived up to the Golden Rule, even though we render aid to only a minuscule fraction of those in need. This is one of the more elementary lessons taught by the case of the starving stranger: that selectivity is an utterly inescapable feature of humane action in a world that overflows with suffering. All of us, no matter how humane, confine our operative sense of responsibility within limits that fall far short of what we could do to alleviate human misery, and it would make a mockery of the idea of intentionality to say that we "intend," consciously or not, all the suffering we leave unrelieved. An immense gap exists between what a literal interpretation of the Golden Rule would seem to require and what even the best of us actually do and consider sufficient, indeed, admirable. What bridges this gap if not convention? Instead of confronting the problem, Davis brushes it aside, insisting that "selection is seldom neutral" and archly advising us to "be skeptical of the excuse that 'we never knew.'"[37]

We would all prefer to believe that our own moral judgments are a pure exercise of reason, shaped only by the immutable nature of mankind and the world and shared by all whose moral judgment is worthy of respect. It is disconcerting to acknowledge that convention, which is liable to vary incommensurably from one cultural and historical situation to another, sets the stage for judgment, establishes its limits, and channels its direction. Yet this is the implication not only of the case of the starving stranger but also of a familiar truism of moral philosophy, "Ought implies can."

To say that "ought implies can" is, obviously, to say that we do not hold people responsible for doing what they cannot do. Less obviously, the truism also means that our sense of what people are responsible for extends no further than our causal perception. At most, we feel responsible only for evils over which we believe we have causal influence (ones about which we "can" do something), and even this is an outer limit, for there are many evils that we obviously could do something to alleviate for which we do not hold ourselves or anyone else responsible. Because convention enters crucially into what we think we "can" do, the dependence of

conception of justice is not its being true to an order antecedent to and given to us, but its congruence with our deeper understanding of ourselves and our aspirations, and our realization that, given our history and the traditions imbedded in our public life, it is the most reasonable doctrine for us. We can find no better charter for our social world. Kantian constructivism holds that moral objectivity is to be understood in terms of a suitably constructed social point of view that all can accept"; John Rawls, "Kantian Constructivism in Moral Theory," *Journal of Philosophy*, 77 (September 1980): 519; *A Theory of Justice* (Cambridge, Mass., 1971).

[36] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 1," 353–59.

[37] Davis, "Reflections on Abolitionism," 804, 802.

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"ought" on "can" carries with it the further implication that convention plays a large role in moral judgment. Cause-and-effect relations pervade our thinking at every level, from high theory to the most mundane affairs of everyday life. They constitute, as one philosopher said, "the cement of the universe."[38] We do not fry an egg, drive to work, or please a lover without drawing on our fund of knowledge about the relation of present acts to future states of the world. But the fund is distinctly cultural. Cause-and-effect relations are not given in raw experience, and what we think we "can" do is very much a matter of interpretation and social convention. Indeed, it is doubly a matter of convention, for convention not only shapes our understanding of cause-and-effect relations (and thus helps define the farthest horizon of our moral vision), it also maps the entire moral landscape within that distant perimeter, specifying which of the many things that we know we could do to relieve suffering we must actually do in order to feel that our lives accord tolerably well with the moral prescriptions of our society. These are what I call our causal horizons.

Let us imagine, for example, that a great earthquake has just occurred, such as that which struck Mexico City in 1985. In a strictly physical sense, I "can" stop writing this essay, fly to Mexico City, and help save at least one endangered stranger's life by lifting debris and performing other emergency tasks. If I took literally the moral rule, "Do unto others what you would have them do unto you," this would seem to be the only acceptable thing to do, for if I were pinned beneath a collapsed building, I would certainly want others to drop their daily routines and come to my aid. Yet I continue working instead of going to the aid of the stranger, and no one accuses me of violating the Golden Rule. Why not? Because, by the prevailing conventions of my time and place, this "can" is not real, not operative. Mexico City is "too far away"; it would disrupt my life "too much." Too far and too much by what measure? Convention supplies the measure. Convention authorizes me to say that I "cannot" help the stranger, at least not in this direct way, even though, in a purely physical sense, I certainly possess the power to do so. This shared, tacit understanding that converts the "can" of physical ability into the "cannot" or "need not" of acceptable moral practice, is a large part of what we mean by convention.[39]

If ought implies can, and "can" is conventional in this sense, it follows inexorably that our concepts of moral responsibility—of what we "ought" to do—are deeply imbedded in social practice and are influenced at least in broad outline by the material circumstances, historical experiences, and technological capabilities of the

[38] J. L. Mackie, *The Cement of the Universe: A Study of Causation* (Oxford, 1974).

[39] One's first instinct is to think that anything that eases our conscience about the suffering of others must be a bad thing and that the path to a better world must lead away from such conventions altogether. But a little thought will show that conventions limiting responsibility in this fashion are utterly indispensable; a world without them is quite impossible to conceive. Even the perfect altruist who devotes his or her life to helping others must choose which victims to help first, and, from the vantage point of the victims not helped, the choice will appear "selective." Some sort of limiting convention is inescapable, but of course that does not prevent us from preferring a more inclusive set of limits than now prevails. A valuable exploration of this and related problems is James S. Fishkin, *The Limits of Obligation* (New Haven, Conn., 1982).

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society in which we live. As our collective circumstances, experiences, and capabilities change, we should expect the limits of moral responsibility to change as well, though not in any simple or automatic manner. The easiest way to illustrate the point is to imagine a radical change in what we "can" do. Obviously, the invention of technology that would enable us to travel to and from Mexico City, or any other scene of disaster, instantaneously and at trivial expense would be very likely to alter the conventions governing moral responsibility in our society, making the passivity that convention now authorizes quite unacceptable, at least in some circles. If we could save someone's life by merely reaching out to press a button, we would be monsters not to do so. A more familiar example of an innovation in institutional "technology" that induces people to make a (nominally) humane gesture they would not otherwise have made is the "Live Aid" rock concert; another, of a much more substantial sort, is the creation of an organization such as Amnesty International that collects funds, publicizes abuses, and provides a new means of exerting influence. Projects of "creative misinterpretation" by large groups of people can have the same effect. Any change in the practices of our society that stretches our causal horizons and expands the sphere within which we feel we "can" act, has the potential to transform what we hitherto perceived as "necessary evils" into remediable ones, thereby exposing us to feelings of guilt and responsibility for suffering that previously aroused only passive sympathy—like the sympathy we feel today for distant earthquake victims who are "too far away" to help. This is the sort of change that I believe accounts for the emergence of the humanitarian sensibility in the eighteenth century.

Because Davis and Ashworth have rival claims of their own to advance about the determining influence that material interest exerts on moral judgment, it is amusing that each wraps himself in the tattered mantle of voluntarism to complain that my mode of explanation is deterministic. Davis throws in for good measure the all-purpose, academic perjorative of our era, "positivistic"—in spite of crediting mankind with much fuller access to positive moral knowledge than I do.[40] I plead guilty to a few overzealous rhetorical flourishes in my original essay that may have exaggerated the explanatory completeness of my scheme, but the scheme itself is not deterministic.[41] The three propositions that "ought" depends on "can," that "can" is largely defined by convention, and that convention is deeply imbedded in social practice—thus making "ought" indirectly contingent to some degree on changing social practices—will no doubt be unsettling to anyone who assumes that moral judgment is an unconditioned exercise of reason aiming at the apprehension of timeless absolutes or Platonic essences. But acknowledging these things does not subvert consciousness or reduce moral choice to a phenomenon of a lower order.

[40] John Ashworth, "The Relationship between Capitalism and Humanitarianism," *AHR*, 92 (October 1987): 820; Davis, "Reflections on Abolitionism," 801, 802, 809–10.

[41] I do not share the curious confidence widespread among historians today that "deterministic" accounts of human affairs must be wrong and "voluntaristic" ones right, as if the question of free will and determinism had been solved once and for all. If my account were deterministic, I would defend it as such. But it is not.

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My scheme of explanation requires only that we admit that moral thinking, like all thinking, is carried on within a mutable framework of assumptions about what the world is and how we relate to it. The scheme is probabalistic rather than mechanistic, and it applies in the first instance to collectivities rather than to individuals. Good Samaritans go back as far as our records go, and I stipulated from the outset that I was not trying to explain their existence. "We are not concerned," I wrote, "with individual episodes of human kindness and decency—which I assume can occur anywhere, anytime—but with a sustained, collective pattern of behavior in which substantial numbers of people regularly act to alleviate the suffering of strangers. That, I take it, is what we mean by the emergence of a new humanitarian sensibility in the eighteenth century."[42] And that is what I set out to explain. Far from denying the authenticity of the reformers' moral outrage or rendering epiphenomenal their choices or values, my purpose has been to protest the transmogrification of all these lively and productive states of mind into monotonous shades of class interest.

If my aim had been to present a full descriptive account of the emergence of antislavery, I would of course have had to write a book instead of an article and devote much more space than I did to Evangelical piety, Quaker earnestness, the ambitions of artisans for independence, and many other subjects. My aim instead has only been to suggest a better way to conceptualize the rich body of evidence and interpretation that Davis and other specialists have already assembled and which I take as the descriptive foundation for my argument.

I have tried to identify a threshold in the perception of personal agency and responsibility, one that helps us make sense of the most striking finding of Davis's first volume on slavery: namely, that before the eighteenth century practically no one, no matter how compassionate or scrupulous, regarded slavery as an intolerable evil. To see the significance of this threshold, it is important to be clear about what was novel in the eighteenth century. The novelty was active opposition to the institution of slavery, based on the conviction that it was an intolerable evil, not recognition that the slave's lot was a bitter one. The suffering of slaves had long been recognized. John Locke, father of liberalism, who invested in the Royal African company and wrote slavery into the Fundamental Constitutions of Carolina, was perfectly aware that it was a "vile and miserable" state of being. As early as the thirteenth century, the authors of *Las Siete Partidas*, a model legal code that specifically authorized slavery and influenced later legislation in the slaveholding societies of Spain and Spanish America, acknowledged that "slavery is the most despicable thing which can be found among men, because man, who is the most noble and free creature . . . is placed in the power of another."[43] The myth of the contented slave of course propped up many a slaveholder's conscience,

[42] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 1," 360.

[43] Herbert S. Klein, "Anglicanism, Catholicism, and the Negro Slave," in *The Debate over Slavery: Stanley Elkins and His Critics*, Ann J. Lane, ed. (Urbana, Ill., 1971), 142. Theoretical considerations aside, the document also contains the observation that the actual condition of Moorish slaves in Spain was "the most miserable that men could have in this world"; David Brion Davis, *The Problem of Slavery in Western Culture* (Ithaca, N.Y., 1966), 103, 118.

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but in every slave regime some people were morally perceptive enough to recognize that slaves suffered—not only when their masters violated local standards of decency but always, even under the best of material conditions, simply because they were "in the power of another." This moral perspective, which recognized that slaves suffered and acknowledged that it was bad for people to suffer and yet tolerated slavery, seems alien to us. Why, in a culture that had long identified humanity with spiritual autonomy, and ethical conduct with reciprocity, did practically no one before the eighteenth century interpret the Golden Rule to require active opposition to the very institution of slavery? Or, to put it another way, why, in a culture that has long honored the Golden Rule, does recognition that slaves suffer have such a long history and opposition to slavery such a short one?

The answer was once sought in the idea of progress, the happy faith that civilization has its own inner dynamic, carrying mankind to ever-higher plateaus of moral knowledge and ability. We have, for good reasons that need no rehearsal here, grown skeptical of the cozy assumption that we moderns abolished slavery because of our superior insight into the requirements of morality. A second possibility lies at the level of explicit appeals, sermons, and speeches. But the answer cannot be found exclusively or even mainly here. There were many such appeals, of course, and they played an indispensable part in mobilizing opposition to slavery. Any fully descriptive account of the history of the antislavery movement would necessarily allot much space to them. Had no one ever preached that slavery was evil, it would be with us still. But to treat explicit preaching against slavery and in praise of benevolence as the sufficient cause of the widening circles of humanitarian reform activity that history records in the century after 1750—and not to ask why such preaching emerged then and why the message was warmly received—would be to beg the most vital questions. Prescription is certainly a proximate cause, but nothing bars us from inquiring into the cause of a cause: what unleashed this avalanche of prescription? What enabled an avalanche of words to trigger in turn a flood of emotion and expressive action? The rule of reciprocity that the Golden Rule embodies is so central to moral judgment that everything else that can be said by way of prescribing moral duty is gilding the lily. The idea of reciprocity was not new; it had been available in the form of biblical precept for at least two millennia, and by itself it provided an adequate *prescriptive* basis for devoting one's entire life to the liberation of slaves. If prescriptions of moral duty had been enough, the history of opposition to slavery would have begun long before the eighteenth century. That it did not suggests that the cause of the sudden surge of humanitarian activism in the eighteenth century was not fundamentally a matter of prescription, no matter how indispensable prescription may have been to the outcome.

A more likely place to look for the answer to our question lies in the conventions that define personal agency, set practical limits to responsibility, and broadly determine specific interpretations of abstract moral prescriptions like the Golden Rule. We cannot literally do for every suffering "other" what we would have others do for us, and without conventional guidelines to limit and specify our respon-

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sibilities, we would not know what it means to "be good." The conventions that prevail in Western culture today enable us to feel that we live by the Golden Rule even though we actually render aid to only a minuscule fraction of the world's needy people—only that fraction so centrally located within our sphere of causal influence that not to help them would seem abnormal, a departure from the level of care that we routinely achieve in our everyday affairs. Our ancestors were also guided by convention, but, because the fabric of everyday affairs in which we are enmeshed is very different from the one they knew, we have no reason to think that the causal conventions that shape our operative limits of responsibility today are the same ones that prevailed two centuries ago. The causal imagination feeds on the recognition that things could be other than they are and that we know how to alter them. Behind every observation that "*x* causes *y*" is a hypothetical annihilation in which *x*, the cause, is imagined not to exist: "in the absence of *x*, no *y*" is the violent fantasy from which each judgment of causal relationship springs. This hypothetical violence comes more readily to us than to our ancestors. We live in a society that is rife with change, that preens itself on the range of options it holds out to each of its members, and that routinely puts in human hands dramatic powers to come, go, move, do, build, destroy, and otherwise transfigure the world that were scarcely dreamed of before the eighteenth century.

If, as I suppose, the conventions that prevailed before the eighteenth century confined what people thought they could do within much narrower limits than ours—if, in other words, their conventionally defined sphere of causal influence excluded much that ours now includes—there would be nothing surprising in their failure to take action against slavery and other evils that we construe as remediable. This difference between their conventions and ours would explain why even the most scrupulous moralists could acknowledge the misery of slaves and even feel a passive sympathy for them without feeling any obligation to take action against the institution that made them miserable—all the while being sincerely devoted to the Golden Rule. Just as causal conventions allow us to have clear consciences today about the starving strangers and earthquake victims whom we omit to help because they are "too far away," so, I suggest, a different set of conventions once made the misery of slaves seem equally far out of reach. Before substantial numbers of people could feel outraged by the very existence of slavery and take action designed to uproot it (which, again, is not at all the same thing as feeling passive sympathy or protesting particular instances of mistreatment, attitudes that leave the institution itself unchallenged), they had to be able to impute to themselves historically unprecedented powers of intervention and to perceive hierarchical social arrangements and institutional structures not as reflections of God's will or manifestations of nature's own order but as contingent, malleable phenomena open to human influence and correction.[44] This is just another, more familiar, way

[44] As Davis observed in his first volume on slavery, the middle years of the eighteenth century "represented a turning point in the history of Western culture. To both religious and secular writers the period brought an almost explosive consciousness of man's freedom to shape the world in accordance with his own will and reason. As the dogmas and restraints of the past lost their compelling force, there was a heightened concern for discovering laws and principles that would enable human

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of saying that an upheaval had to occur in the conventions governing the attribution of causation and responsibility. One result of upheaval would have been to transform what had been perceived as "necessary evils" into remediable ones, thereby imposing on certain sensitive souls a compelling sense of obligation to remedy miseries that had in all prior human history evoked no more (and usually less) than passive sympathy, even from the most scrupulous and compassionate moralists.

Does this scheme of explanation make human beings "passive automata wholly programmed by 'society,'" as Davis suggests?[45] Certainly not. We do not detract from the honor due those hardy souls who first perceived slavery as a remediable evil by observing that equally compassionate people in previous centuries did not respond to the same facts in the same way, or by suggesting that people could not have responded as the reformers did until they felt, accurately or not, that they had knowledge of cause-and-effect relations sufficiently complex and far-reaching that they could readily imagine a course of practical action capable of uprooting one of the most ancient and interest-bound institutions in their society. To say that it took a shift of causal conventions to push people over this threshold and place slavery (and much else) on the agenda of remediable evils, making possible the collective action historians call "humanitarianism" does not reduce humane sentiments to something lower or undermine in any way the subjective sense of freedom to choose between perceived options that we all normally feel. The only vantage point from which my scheme must appear comparatively deterministic is that of a reader who (boldly) believes that mankind is free not only to choose between perceived options but also to choose which options to perceive, for my causal explanation extends only to what people perceived the outer limits of their responsibility to be, not to the choices they made in light of that perception.[46]

To contend that the moral perceptions that were constitutive of humanitarianism did not exist before the eighteenth century and could not have become available until certain historical conditions were met is no more deterministic, in the ordinary sense of that word, than observing that a person cannot jog while seated, play bridge without a deck of cards, perform a lifesaving tracheotomy without medical training, or understand what inflation is without having lived in a monetary economy. To ask that our moral judgments and perceptions be

society to be something more than an endless contest of greed and power. This quest for moral assurance led inevitably to examinations of inequality, sovereignty, and servitude"; Davis, *Problem of Slavery in Western Culture*, 485.

[45] Davis, "Reflections on Abolitionism," 802.

[46] Ashworth fears that my scheme falls victim to "technological determinism" because it does not allow for the possibility that people might first feel responsible for suffering and then seek means (technologies) for doing something about it. Ashworth fails to recognize that no one can "feel responsible" (operatively responsible, as opposed to feeling passive sympathy) for suffering whose knowledge of cause-and-effect relations is so limited that he or she cannot imagine any course of action that would relieve the suffering. Responsibility presupposes possession of some threshold level of recipe knowledge, and the entire point of my scheme is to account for the crossing of that threshold. It is of course true that once a person feels responsible, he or she is then free, within limits, to seek appropriate means to relieve the pain, but the very fact of feeling responsible means that the critical threshold that should interest us historically has already been crossed. See Ashworth, "Relationship between Capitalism and Humanitarianism," 820.

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independent even of conditions like these would be to ask for a sort of freedom that the world cannot deliver and that we do not need.

NOTHING HAS SEEMED MORE SHOCKING TO MY CRITICS than my suggestion that the capitalist marketplace had something important to do with the establishment of these cognitive preconditions for the emergence of the humanitarian sensibility. Their astonishment is ironic, since the role I assign to capitalism is a good deal smaller and less direct than the one Davis assumes. Instead of treating the antislavery movement (or at any rate, its acceptance by the powerful) as a refraction of class interest, unconsciously aiming at the legitimation of a new, ostensibly free, system of labor discipline, I argue that the market merely had the incidental effect of expanding the sphere of causal perception within which everyday affairs proceeded, pushing people over a threshold of perception such that the most sensitive moralists among them no longer found passive sympathy an adequate response to the misery of slaves. The point of departure for my argument was the conviction prevalent among historians that the concomitance of capitalism and abolitionism, though paradoxical, was not coincidental. Taking for granted the existence of some connection, I set out to rethink what it might be, other than class interest. My conclusion that capitalism contributed to the freeing of the slaves "only a precondition, albeit a vital one" does not assign capitalism a very large role in the emergence of the humanitarian sensibility.[47] Had there not already been a debate in progress over the relationship between capitalism and antislavery, I am not at all sure that I would have assigned capitalism a role even that large, for, unlike many of the participants in this longstanding controversy, I have no interest at all in chalking up points "for" or "against" capitalism.

From my perspective, the question that most needs answering concerns the history of causal perception: why did social problems that once appeared intractable take on, in the eighteenth century, an appearance of unprecedented plasticity? Capitalism enters the picture only insofar as it helps answer that question. After centuries during which the misery of slaves was treated with indifference, or at most as a necessary evil, something produced an outward shift in the conventionally defined sphere of causal perception, such that some unusually acute moral reasoners began finding it easy to imagine a course of action leading to the destruction of slavery—so easy that not to embark on it seemed a dereliction of moral duty. What was it that produced this crucial expansion of the sphere of causal perception within which people assessed their responsibility for the suffering of strangers?

My crucial claim in this regard is that the market was one of the major factors in the expansion of causal horizons. Pivotal though the claim is, Davis and Ashworth put up surprisingly little resistance to it. As Davis says, "there can be no doubt that the market tended to teach people to keep their promises and attend

[47] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 2," 563.

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to the remote consequences of their acts."[48] This concession is more important than Davis recognizes. Given the dependence of "ought" on "can," and "can" on conventions of causal attribution, the tendency of the market to widen causal horizons gives us a way of accounting for the concomitance of capitalism and abolitionism that relies neither on naive concepts of moral progress nor on class interest cunningly disguised as altruism. Capitalism and humanitarianism seem antithetical—and their concomitance puzzling—because of the divergence of the sentiments on which each rests. Capitalism fosters self-regarding sentiments, while humanitarianism seems other-regarding. What can account for the parallel development in history of two such opposed tendencies? The mystery fades considerably once we recognize that, in spite of their divergent properties, capitalism and humanitarianism also have something important in common: both presuppose the existence of wide causal horizons. Both depend on people who attribute to themselves far-reaching powers of intervention. Neither can flourish unless it can enlist the energies of people who display a strongly self-monitoring disposition, people who routinely allow their behavior in the present to be shaped by obligations incurred in the distant past and by anticipations of consequences that lie far in the future. People who dwell only in the present and attribute to themselves little power to alter the course of events live in a world that cannot sustain either a market-oriented form of life or the acute sensations of moral responsibility that Nietzsche derisively associated with the "bad conscience" of the humanitarian reformers. And, although humanitarianism has no means of bringing into existence the type of "conscience-ridden" personality it needs, capitalism, through the disciplinary mechanism of the market, has a very considerable power to do just that.

It would, of course, be absurd to credit the market with the power of bringing this personality type into existence by itself. The point of my previous article was to show that capitalism and humanitarianism both presuppose a population imbued with the habit of remote causal attribution and therefore that their concomitance can be accounted for without construing humanitarianism as a reflex of class interest. I did not claim that capitalism was the sole source of the humanitarian sensibility. Humanitarianism and the personality capable of sustaining it were the outcome of a "civilizing process" that extended over many centuries and resulted at least as much from religious and political developments as from anything economic. Christianity's definition of the person as a potentially immortal, but immaterial, soul, temporarily housed within a corruptible, material body, is eloquent testimony to the antiquity and the centrality in Western culture of the aspiration toward overcoming the self and transcending impulse. No market ever held out any reward for delay of gratification that could compare with that which Christianity held out to those who could learn to live for the future: eternal salvation.

At a completely different level of analysis, Norbert Elias has made a strong case for the importance of geographically extensive monopolies of physical force. Only

[48] Davis, "Reflections on Abolitionism," 811.

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the emergence of such monopolies in the medieval period, he contends, permitted the crystallization of networks of functional dependence within which social constraints could achieve enough force and steadiness to be internalized as self-restraint. "The moderation of spontaneous emotions," he argued, "the tempering of affects, the extension of mental space beyond the moment into the past and future, the habit of connecting events in terms of chains of cause and effect—all these are different aspects of the same transformation of conduct which necessarily takes place with the monopolization of physical violence, and the lengthening of the chains of social action and interdependence. It is a 'civilizing' change of behavior."[49] The political rigors of living at court and taking part in courtly intrigue no doubt did more to convert warriors into calculating courtiers than either religion or any economic institution of the age could, and yet Elias recognizes that what life at court once did to induce foresight in a handful of warrior aristocrats was, in the eighteenth and nineteenth centuries, extended to a much larger sector of society by the trade networks and functional dependencies of an increasingly interdependent economy. Even then, of course, the market could shape personality to its needs only within limits established by traditional religion and morality and only within a framework of legal order and comparative political stability supplied by the state.

The market reinforced the long-term drift of Western culture toward vigorous self-surveillance and far-flung causal horizons, and these tendencies were magnified still further by contemporary events such as the scientific revolution of the seventeenth century, the dramatic technological feats of the next two centuries, and the eruption of political revolutions in France and North America. Nothing could have done more to stimulate the causal imagination or to dispel illusions of immutability (sides of the same coin) than the sight of a world of ranks turned topsy-turvy. But revolutionary acts presuppose a high degree of confidence that the world is open to alteration, and the humanitarian sensibility did not make its first appearance in 1776 or 1789. As Karl Marx observed, capitalism had long been teaching, in a subtle yet compelling way, much the same lesson about mutability that revolution taught: "All that is solid melts into air, all that is holy is profaned."[50]

Although Davis and Ashworth accept the idea that the market helped expand causal horizons, they seem not to understand that this effect in itself sheds much light on the concomitance of capitalism and humanitarianism and establishes a linkage between "base" and "superstructure" that does not rely on the concept of interest. I contend that the market, by expanding causal horizons, helped bring into existence a perceptual world in which some people—those who would have been distinguished for their exceptional conscientiousness in any case—began defining their own responsibility for suffering so broadly that they, unlike their equally scrupulous predecessors in earlier generations, could no longer witness the misery of slaves with merely passive sympathy. Attributing to themselves great

[49] Norbert Elias, *Power and Civility: The Civilizing Process*, vol. 2, Edmund Jephcott, trans. (New York, 1982), 236.

[50] Marx quoted in Marshall Berman,"'All That Is Solid Melts into Air': Marx, Modernism, and Modernization," *Dissent*, 25 (Winter 1978), 54–73.

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powers of intervention, they also found themselves exposed to acute sensations of guilt and responsibility-by-omission for evils whose causes had previously seemed inaccessibly remote to everyone, even the most sensitive moralists.

In contrast to this explanation, in which the idea of a perceptual threshold figures prominently, Davis and Ashworth seem to believe that, in my view, the market acted unilaterally and mechanically to bring about a general improvement in the morality of those subject to its influence, converting sinners into saints. They impute to me a theory in which the market functions like a source of benign radioactivity: the closer a person is to it and the longer exposed, the greater the dosage of humane impulses—no matter what the person's values, character, religious convictions, ethical commitments, gender role, material interests, or state of mind. This is the theory that Davis evidently has in mind when he contends that my scheme cannot explain the presence of women in the antislavery movement (because they were comparatively uninvolved in the market) and suggests that my theory should lead us to expect slavetraders to have been converted to the antislavery cause (because they were directly involved in market transactions).[51]

I took considerable pains in my original article to guard against this sort of misinterpretation, especially in the "case of the starving stranger," where the idea of humanitarianism as a threshold phenomenon is developed at some length. It was the "radioactive" view of the market's function that I was rejecting when I wrote that "the argument presented here is not that markets breed humane action but that in the particular historical circumstances of late eighteenth-century Anglo-American culture the market happens to have been the force that pushed causal perception across the threshold that had hitherto made the slaves' misery (and much other human suffering) seem a necessary evil."[52] That was also the view I meant to reject when I stressed that John Woolman's pioneering antislavery work presupposed both exceptionally wide causal horizons and exceptional moral standards and yet again, when I acknowledged that most of those whose horizons expanded took the occasion only as an opportunity to pursue self-interest at a higher pitch.[53]

That slavetraders did not become abolitionists and that women often did would tend to disconfirm an explanatory scheme which held that markets uniformly induce humane behavior in those most closely exposed to them, regardless of all other influences. But the scheme I set forth holds no such thing. In view of the longstanding identification of femininity with compassion and moral recititude, I see nothing at all surprising or disconfirming about the prominence of women in the antislavery movement (or about the prominence of people of deep religious convictions, of a comparatively rigid moral temperament, or of a fairly high level of aggressiveness and self-confidence). For women as well as men, the key question remains why indifference and passive sympathy for slaves gave way in the eighteenth century to active opposition to the institution of slavery. Anyone

[51] Davis, "Reflections on Abolitionism," 812.
[52] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 2," 563.
[53] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 1," 353–59; "Capitalism and the Origins of the Humanitarian Sensibility, Part 2," 565–66, 562–63.

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seeking the answer to that question would have to take the stereotype of the separate spheres very literally indeed to think that women were not influenced by the market (or that men were not influenced by the domestic sphere). To say as Davis does that women "were not subject to . . . market discipline" is to construe the cultural impact of the market too narrowly.[54]

As for the idea that the market had the power to transform callous people into ethically sensitive ones, or slavetraders into abolitionists, I am unable to find any statement in my essay remotely capable of encouraging such a preposterous inference. Of course, the market did not exist in a vacuum or act on its subjects unilaterally—nothing in the universe of human affairs does. The market was only the keystone for an entire way of life, one in which radicals could be as deeply immersed as entrepreneurs and about which women could become as conversant as men. Like other complex institutions, the market determined nothing rigidly but channeled conduct by encouraging some perceptions and discouraging others. Like poems and rainy days, the experiences it induced were open to a range of interpretations, and the interpretation given to them varied with the personal history of the interpreter. Quakers, prosperous artisans, and other members of the middle class were among the earliest to adopt this way of life, and it is no coincidence that it was from among these groups that some of the earliest antislavery leaders emerged, but a person did not have to cut deals, sign contracts, dispatch bills of lading, or mobilize a work force in order to see the world in an altered perspective and experience the heightened sense of agency that is relevant to my form of explanation. Nor today is it necessarily airplane pilots and computer operators who best grasp the revolutionary cultural implications of the innovations they superintend. The market did not make anyone "good." The market does not help explain why some people conducted themselves morally but why moral conduct was redefined in some circles to include root-and-branch opposition to slavery, a problem previously construed as intractable.

Of the three efforts Davis makes to test my thesis empirically, the first two, concerning women and slavetraders, seem to me very wide of the mark, but the third is more interesting. Assuming that Holland was a strongly market-oriented society, Davis takes the weakness of the antislavery movement in Holland to be an argument against my thesis. His point is well taken: if the advance of a market-oriented way of life is what pushed Britons and Americans over the threshold into a new moral universe, why was the same effect not felt in Holland? The Dutch case is well worth further exploration, but we must begin by noting that it poses no more of a problem for me than it does for Davis. He, after all, *should* find it puzzling that the Dutch bourgeoisie passed up the opportunity to legitimize wage labor, accumulate moral capital, and bolster its own self-esteem by attacking

[54] Davis, "Reflections on Abolitionism," 812. To see how easy it would be to exaggerate the gender specificity of the market's impact on culture, one need only think of the centrality of the concepts of covenant and contract in English political philosophy and religion. A recent important work on religion is David Zaret, *The Heavenly Contract: Ideology and Organization in Pre-Revolutionary Puritanism* (Chicago, 1985). For a very broad construction of the cultural impact of the market, see Jean-Christophe Agnew, *Worlds Apart: The Market and the Theater in Anglo-American Thought, 1550–1750* (New York, 1986).

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slavery. If Anglo-American capitalists were unconsciously attracted to antislavery agitation because it "gave a certain moral insulation to economic activities less visibly dependent on human suffering and injustice," why did Dutch capitalists not feel the same attraction?[55]

From both Davis's vantage point and my own, the Dutch case can fairly easily be distinguished from the Anglo-American one by pointing to the decidedly paternalistic temper of Dutch political life and to the very marginal role that slave labor played in the Dutch economy. Relying only on the same essay that Davis cited by the Dutch historian P. C. Emmer, we can see that the slave trade to the Dutch colonies in the New World had nearly ceased as early as 1773, not because of agitation but because of diminishing returns. About 36,000 slaves lived under Dutch control when emancipation came in 1863, and in the 1840s the trade between the principal slave colony, Surinam, and the Netherlands amounted to only about 1.5 percent of the Dutch gross national product. Even when Dutch consumers drank slave-grown coffee sweetened with slave-produced sugar, they were relying largely on imports from plantations beyond the control of their government. In the interval running from about 1800 to 1813, slavery was a "purely academic issue in the Netherlands" because during those years all of the Dutch slaveholding colonies were controlled by the British.[56]

There may be some sense in which the Dutch deserve their reputation as a market-oriented people, but, if so, their political culture in the era of abolition was curiously lacking in the liberalism that is generally taken to be the paramount political expression of market culture and capitalist hegemony. Emmer reported that "the ideology of political and economic liberalism found few adherents in the Netherlands" and noted that in 1840 the king was so determined to resist popular initiatives of any kind that he refused even to receive a petition by the British Anti-Slavery Society—one that was accepted by all the other monarchs of Europe except the Sultan of Turkey. The king seems to have been not so much concerned to defend slavery as to retain control over its eradication, for, although the details of compensation were drawn out, emancipation was taken for granted as an inevitability by the government after 1844 and by slaveowners as well after 1852.[57] My thesis would be severely challenged by evidence of a truly market-oriented society whose members continued to see slavery as nothing worse than a necessary evil, but that is not what the Dutch represent. On the contrary, support for slavery seems to have evaporated in spite of the absence of any strong, indigenous antislavery movement. The Dutch case is an odd one, and further investigation

[55] Davis, *Problem of Slavery in the Age of Revolution*, 251. Davis might argue that it was not capitalists generally who sought to legitimize wage labor but industrial capitalists, and that the Dutch bourgeoisie was predominantly mercantile. Before one could assess such an argument, one would need to know more about the Dutch economy than has been brought forward so far. Merchants need wage labor, too, and it would not be at all easy (judging from what Davis has written in *Problem of Slavery in the Age of Revolution*) to show that Anglo-American abolitionists owed more to industrial than to mercantile wealth.

[56] P. C. Emmer, "Anti-Slavery and the Dutch: Abolition without Reform," in *Anti-Slavery, Religion, and Reform: Essays in Memory of Roger Anstey*, Christine Bolt and Seymour Drescher, eds. (Folkestone, Kent, 1980) 80–83, 88–89, 84.

[57] Emmer, "Anti-Slavery and the Dutch," 85, 87, 80.

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may demonstrate that it is a genuine counterinstance, but the evidence brought forward thus far is too ambiguous to do my thesis any significant harm. Indeed, in the absence of political agitation, what else could account for the abandonment of slavery in the Dutch colonies if not a pervasive sea change in moral perspective?

Momentarily shedding his antipositivist robes, Davis complains in the closing pages of his essay that my form of explanation "has no predictive power." Frustrated by my untroubled acknowledgment that not everyone exposed to the market was transformed by it into an abolitionist—indeed, that for many people an expansion of causal horizons meant only that self-interest would be pursued on a grander scale—Davis says that "this very softness shields Haskell from serious empirical attack." For Davis, it is a sign of the lamentable evasiveness of my explanation that "Haskell can claim that nothing is proved by the failure of slavetraders to become humanitarians."[58]

This complaint reflects not only a failure to recall what the argument is about—why two developments as dissimilar as capitalism and humanitarianism emerged in close historical conjunction—but also a failure to grasp the character of a threshold explanation. Surely, Davis does not really expect historians to explain the conduct of human beings more fully than a chemist can explain the motion of molecules—yet, when a chemist explains the boiling of a pot of water by saying that at certain threshold values of pressure, volume, and temperature, water will begin to undergo a change of state, becoming a gas instead of a liquid, we do not reject the explanation because some of the water in the pot remains a liquid, nor do we complain about the chemist's failure to tell us which individual molecules will break through the surface and enter the atmosphere—it is enough to know that some (those with the greatest velocity) will do so.

Although we have no analog in the study of human affairs to the thermometers, gauges, and scales that enable the chemist to predict when the change of state will begin, that does not prevent us from realizing that we are dealing with a threshold phenomenon and that it was only natural for the line to be crossed at a time when the market was powerfully reshaping everyday life to its needs. Being skeptical about moral progress, scholars must assume that people who lived before the eighteenth century were about as insightful and capable of moral choice as people are today. We know that they recognized that slaves suffered and also that they acknowledged the force of a moral prescription that required them to do for others what they would want done for themselves. I am not trying to account for an instantaneous or universal triumph of antislavery opinion, for, even at the moment of emancipation, some people favored the retention of slavery, many were indifferent, and most probably continued to feel only the passive sympathy that had for so many centuries marked the outer limits of response. Missing from this picture, however, is an expansion of the perceived limits of human agency and responsibility—one sufficiently far-reaching to account for the emergence of a determined network of activists and a gradual shift of opinion in their direction. Without attributing to the market any power whatsoever to make bad people good,

[58] Davis, "Reflections on Abolitionism," 811.

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we can see that the premium the market paid for forethought and all the other habits of remote causal attribution could easily have supplied the missing piece, completing the picture, and creating a world of wide causal horizons in which slavery would appear to some people as a remediable and intolerable indignity.

What most disturbs both Davis and Ashworth is, perhaps, not the supposed determinism or limited predictive capacity of my explanation but rather its counterintuitive character. Davis's own account has a counterintuitive touch insofar as it depicts bad capitalism giving rise to good antislavery sentiment, but then its final twist, in which antislavery is shown to be shot through with hegemonic implications, confirms the reader's intuitive expectation that bad causes should produce bad effects. In contrast, my explanation requires the reader to believe that the market had one decidedly good effect, and this idea will be counterintuitive for those who are committed to the view that the market is a bad thing. Up to a point, I have no quarrel with those who criticize the market. As I acknowledged very fully in my original essay, "the market" was the favorite rhetorical resource for several generations of people intent on playing down concerns about the public interest and legitimizing the pursuit of self-interest. There may be a touch of romanticism in Davis's lament for the "older, paternalistic notions of responsibility" (which could, of course, be extremely intrusive and never hinted of any general obligation to help strangers), but there can be no doubt that appeals to the automatic functioning of the market often served to justify cold and callous relations between contracting parties, including especially those between employers and employees.[59] If the market shrank responsibility in this respect, my critics ask, how can it be said to have expanded responsibility in any other?

The answer is obvious: complex institutions like the market have multiple and contradictory effects that defy any sort of reconciliation. So, for that matter, does so common a substance as water, which cooks or cools, gives life or takes it away, enables distant transport or destroys whole towns and villages, depending on its amount, its temperature, its relation to bodily orifices, and so on. Why would anyone expect the effects of the market to be any less diverse and contradictory than those of water? And what point would there be in trying to sum up its overall impact, as if each bad effect came tagged with a minus sign and each good one with a plus? Does the scalding of John somehow cancel out or negate Mary's cooling bath? Or a thirst-quenching drink make up for a devastating flood? Because these effects have no common denominator, we have no choice but to deal with them discretely, which means that bad causes may well lead to good effects.[60]

Both of my critics seem to fear that by saying something good about the market we forfeit the right to indict the evils associated with it. I give us all greater credit for mental agility than this fear implies. Ashworth is even more insistent than Davis that the good effects I have attributed to the market could only be justified by some sort of calculus that would tote up all the market's good and bad results and show

[59] Davis, "Reflections on Abolitionism," 811.

[60] Compare with Ashworth's complaint that Woolman's humane sentiments are "negated" by the example of Daniel Defoe: Ashworth, "Relationship between Capitalism and Humanitarianism," 818.

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that its overall impact on morality was good.[61] The project appears to me both futile and irrelevant. There is nothing illogical about attributing to the market two distinct effects: it authorized the more aggressive pursuit of self-interest in business affairs and, by expanding the horizon of causal perception, it also encouraged in some people strong feelings of guilt and anger about suffering that had previously aroused no more than passive sympathy. One would not be surprised to find individuals in whom both effects were expressed simultaneously and whose behavior toward others might accordingly meander between rigorousness in regard to people perceived as contractual equals and tenderness in regard to those—like slaves—who were perceived as suffering through no fault of their own. This is, in fact, not a bad description of a common Victorian stance toward others. Knowing that, by some overall measure, the market was, on the whole, good or bad would add nothing to our understanding of this contradictory scene. We know that water is, on the whole, good, in that life cannot continue in its absence. But this awareness does not diminish the importance of knowing that floods, drownings, and scaldings are bad. By the same token, even if we were to conclude that the market's effects on the whole were bad, this would not diminish the importance of knowing that one of them was good.

ALTHOUGH I HAVE TRIED TO DEMONSTRATE THAT the humanitarian sensibility can be linked to capitalism without relying on the much-overworked concept of class interest, I took pains in my original article to acknowledge that interest functioned as an indispensable concept in human affairs, and I noted in particular that "interests exert an important influence on belief through what [Max] Weber called 'elective affinity.'"[62] My aim has not been to supplant the concept of interest—an unthinkably bizarre project—but to supplement it and suggest that its explanatory power may be exaggerated. In his response to my essay, John Ashworth often seems to assume that my disapproval of Davis's sort of explanation, which wraps interest in a cloak of self-deception, must extend to all forms of explanation based on interest, including his own. But this is not quite so. I find much to agree with in Ashworth's essay. In its best moments, it bears a resemblance to Weber's approach, which, while denying the crude thesis that interests directly determine consciousness, acknowledges that, through the process of "elective affinity," people do adopt ideas and values that are loosely suited to their interests. Ashworth's understanding of the role of interest is not consistent, and I am not at all persuaded by his claim that antislavery can best be understood as a product of class interest, but my objections have more to do with execution than the type of explanation he is recommending.

In a curious maneuver near the beginning of his argument, Ashworth nearly succeeds in obscuring its merits (and its kinship with that of Weber) by announcing that his aim is to rehabilitate the discredited notion of "false consciousness." This

[61] Ashworth, "Relationship between Capitalism and Humanitarianism," 817.

[62] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 1," 342; Richard Herbert Howe, "Max Weber's Elective Affinities: Sociology within the Bounds of Pure Reason," *American Journal of Sociology*, 84 (1978): 366–85.

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term has a very checkered history and no certain referent, but it is often associated with the very view Weber was attacking, one that treats consciousness as epiphenomenal. This, it turns out, is not at all what Ashworth means. He means by the term only that historical actors always act on an "incomplete" perception of reality (never mind all the questions that could be raised about the singularity and objectivity of "reality" and the "completeness" of our own perception) and that historians are entitled to assign to historical acts meanings other than those the actors had in mind.[63]

Why Ashworth maintains that the assertion of these familiar propositions requires the resurrection of false consciousness is not clear to me. He evidently imagines that I wish to confine the meanings of acts to those which the actors themselves held, but this is simply not true. When I explain humanitarianism in terms of a shift of causal conventions, I am obviously assigning to the actions of the reformers meanings very different from the ones they had in mind. Ashworth apparently mistakes my concern with cultural availability for the outlandish view that only the actor's meanings count. It is of course vital to ascertain the range of meanings culturally available to past actors so that we do not mistakenly impute to them a view of the world and an understanding of self that they could not have had. But this concern to avoid anachronism in no way prevents us from contrasting the meaning the act had for the actor with the meaning that it has for us, which may be very different because we view it in retrospect and see it in light of our different experiences and priorities. Indeed, ascertaining cultural availability is the indispensable groundwork for any such distinction, and only by maintaining this distinction scrupulously can we prevent history from being a bag of tricks played on the dead.

Disregarding, then, the unfortunate connotations of false consciousness, we can see in Ashworth's proposal a scheme of explanation that bears comparison to the approach that Weber developed most fully in *The Protestant Ethic and the Spirit of Capitalism*.[64] Weber did not employ the notion of self-deception and neither does Ashworth, who is no more persuaded by this aspect of Davis's interpretation than I am. Ashworth agrees with Davis, as I do, that some of the consequences of reform served the hegemonic interests of the reformers, but unlike Davis, he treats these consequences as unintended—just as I do. Neither consciously nor unconsciously, in Ashworth's view, did any significant number of reformers intend, by attacking slavery, to advance their own interests. Here again, his treatment parallels that of Weber, who insisted on a very close relationship between the spread of Protestant religious doctrines and the advance of capitalism but who strongly denied that ministers or their parishioners had ever *intended* anything other than the glorification of God.

It is at the next stage of the argument that Ashworth parts company with both Weber and me. Although he does not regard the hegemonic consequences of reform as in any sense intended, Ashworth does believe that they must be viewed

[63] Ashworth, "Relationship between Capitalism and Humanitarianism," 814–15.
[64] Max Weber, *The Protestant Ethic and the Spirit of Capitalism*, Talcott Parsons, trans. (New York, 1958).

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as a product of class interest. In contrast, I had argued that the very idea of "pursuing class interest," or being "moved" by it, implies intentionality, at least of an unconscious character. "To say that a person is moved by class interest is to say that he *intends* to further the interests of his class, or it is to say nothing at all."[65] This was a strong assertion and not one I would want to detach from its original argumentative context and set up as a general methodological imperative, but it does accord with Weber's practice in *The Protestant Ethic*: in his view, the contribution that Protestantism made to the advance of capitalism was neither intended nor the result of class interest. Weber held that it was a fortuitious conjunction, not any causal dependence of values on interests, that brought capitalism together with a religious ideology capable of breaking down the traditional psychological barriers that had hitherto blocked economic advance.[66]

Ashworth says at one point, in a very un-Weberian manner, that "a person is moved by certain ideals that have *grown out of* class interest" (emphasis added).[67] If the relationship between interests and values was truly like that of the acorn to the oak or the caterpillar to the butterfly, so that one grows out of the other, we would have grounds for saying that interest is the "major factor" in explaining both values and whatever outcomes they lead to in action. But the metaphor "growing out of" notoriously exaggerates both the objectivity of interest and the determinateness of the influence that it exerts on our thinking. It was in part to protest that sort of exaggeration that Weber turned to the much looser and more ambiguous metaphor of "elective affinity."

The question of how much class interest explains is finally one of degree, for neither Weber nor I deny that interests and values tend to go together and to be mutually reinforcing. In fact, my own scheme, by treating people's perceptions of both interest and moral obligation as elements of the cognitive style appropriate to a market-oriented way of life, suggests a connection between values and interests that is even more intimate than the word "affinity" implies—namely, a common origin in a set of causal conventions. But I do not believe that the values of the movement against slavery were generally produced by class interest or that the movement can be best explained in its terms. And if we focus on Ashworth's practice rather than his methodological pronouncements, neither does he—at least, not all the time.

Ashworth says that he is going to show, contrary to my argument, that the reformers were moved by class interests, even though they had no intention of advancing those interests. But the scene he actually paints for us depicts people being moved not by class interests but by consciously held values and ideals, such as the importance of personal autonomy, the primacy of conscience, the sanctity

[65] Haskell, "Capitalism and the Humanitarian Sensibility, Part 1," 347.

[66] "For those to whom no causal explanation is adequate without an economic . . . interpretation, it may be remarked that I consider the influence of economic development on the fate of religious ideas to be very important and shall later attempt to show how in our case the process of mutual adaptation of the two took place. On the other hand, those religious ideas themselves simply cannot be deduced from economic circumstances"; Weber, *The Protestant Ethic and the Spirit of Capitalism*, 277 n.84; see also chap. 2.

[67] Ashworth, "Relationship between Capitalism and Humanitarianism," 815.

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of the family, and the like. True, Ashworth makes a case for the idea that these values and ideals were, in turn, related to the class interest of the reformers, but the relation he draws is not very strong, and, even if we accepted his claim at face value, it would not follow that the reformers, in attacking slavery, were "pursuing class interest" or that antislavery could be best understood as an expression of class interest. That antislavery values and bourgeois interests are not wholly unrelated goes without saying, as does the existence of affinities between them. But that is not what the debate is about. Ashworth has evidently set out to prove that class interest is "the major causal factor," the principal explanation of the movement against slavery, and in this he does not succeed.[68]

Consider the structure of his argument. He contends that the commodification of labor that attended the advance of capitalism provoked, in the middle and upper levels of society, a twofold reaction: a heightened ideological commitment to a cluster of values centering on the priority of individual conscience and the sanctity of the family and, in close association with these values, a general search for ways of setting limits to the wholesale legitimation of self-interest that the triumph of capitalism seemed to threaten. Because a strong conscience was commonly thought capable of anchoring moral judgment in the bedrock of divine law, and home and family were widely understood to be refuges from competition within which disinterested love could survive and flourish, these values were, appropriately enough, held aloft by those leading the battle to confine self-interest within manageable boundaries. As these ideological commitments gathered strength, the institution of slavery, which disrupted families and substituted the self-indulgent whims of a slaveowner for the sacred authority of conscience, came to appear ever more evil and anomalous. Finally, inspired by the vision of bounded liberty and conscientious conduct that the struggle to come to terms with wage labor had indirectly set in motion, the middle and upper classes felt obliged to rid their society of slavery, in what might be described as a paroxysm of ideological consistency. The ideals of family and conscience that they had come to venerate they sought to translate into reality, not only in their own lives, which would be unsurprising, but also in the lives of the slaves—which Ashworth should find much more surprising than he does.

In fact, I find this explanation unpersuasive (at least in its present, abbreviated form) on several different levels. Ashworth fails to establish a strong enough connection between class interests and ideology, on the one hand, and between ideology and antislavery action, on the other, to justify his conclusion that antislavery ought to be understood mainly as an expression of class interest. Still more vulnerable are certain of his assumptions. He imputes to human beings a greater thirst for consistency between values and practices than I believe they display, and he correspondingly underestimates the ease (and sincerity) with which even the most passionately held values can be interpreted so as to seem in conformity with a wide variety of practices. Most serious of all, Ashworth offers no explanation for what I regard as the defining characteristic of the rise of

[68] See Ashworth, "Relationship between Capitalism and Humanitarianism," 823–28, 815.

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humanitarianism as a historical event—the extension to strangers of levels of care and concern that were previously confined to family, friends, and neighbors. No doubt slavery was an outrage against ideals of conscience and family, but it was an outrage perpetrated against the slaves, not the reformers whose behavior we are trying to explain. The reformer's own domestic tranquility obviously was not imperiled by the existence of slavery and neither was the reformer's fidelity to conscience, except insofar as he or she chose to construe the extension of benevolence to slaves as one of the demands of conscience. Ashworth gives us no reason to think that reformers could not have cultivated these values within their own circles, while leaving slaves bereft of them, just as democratic and republican values had been cultivated within the free populations of ancient Athens and colonial Virginia—or, for that matter, just as the values of hearth and home were cultivated on many an antebellum plantation. By taking it for granted that people who learn to venerate conscience and the family will naturally seek to extend those blessings from those who are near and dear to the most lowly and distant of strangers, Ashworth not only simplifies his task, he literally assumes away the central problem for which we seek an explanation.

This is not to say that Ashworth's account is without merit. I strongly agree with his suggestion that abolition was part of a broader effort to tame the market by setting limits to the pursuit of self-interest. Ashworth evidently imagines that the existence of such an effort is evidence against my thesis, but my argument has never depended on the claim that the preponderance of the market's effects on morality was good. I claim only that the market helped push people across a critical threshold of perception; whether it was on the whole good or bad may be a lively political question, but it is irrelevant to the question of historical causation that concerns us here. Having myself written about some of the quite different forms that this countermovement against the market took in the latter half of the nineteenth century, I can heartily endorse Ashworth's suggestion that the celebration of conscience, the veneration of family life, and the repudiation of slavery ought to be viewed as three aspects of a many-sided campaign to restore moral authority in the face of the market's de-moralizing impact on traditional practices.[69] Much of the history of England and America in these years could be written in terms of this wide-ranging effort at containment, some aspects of which were first described by Karl Polanyi in *The Great Transformation*, a book Ashworth does not mention but in which he would find much support for this part of his argument.[70]

Although I agree that the ideology of antislavery drew strength from these concerns about the widening orbit of legitimized self-interest in capitalist society, the relationship between this ideology and the class interests of those who

[69] Ashworth, "Relationship between Capitalism and Humanitarianism," 822. See my essay "Professionalism versus Capitalism: R. H. Tawney, Emile Durkheim, and C. S. Peirce on the Disinterestedness of Professional Communities," in *The Authority of Experts: Studies in History and Theory*, T. L. Haskell, ed. (Bloomington, Ind., 1984), 180–225; and *The Emergence of Professional Social Science: The American Social Science Association and the Nineteenth Century Crisis of Authority* (Urbana, Ill., 1977).

[70] Karl Polanyi, *The Great Transformation* (New York, 1944).

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embraced it is much more ambiguous than Ashworth allows. In a footnote, Ashworth applauds a statement I made in my original article warning that the concept of interest is elastic—so elastic that there is literally no human act that cannot be construed as self-interested.[71] But he proceeds inadvertently to formulate what deserves to become the classic textbook illustration of the dubious practice I was warning against, by contending that a campaign to inhibit the pursuit of interest was itself an expression of interest. What can one say? Interest can never be ruled out as an explanation of behavior: measures taken to restrain self-interest may in fact have benefited some people more than others, and it is not inconceivable that some of the beneficiaries—whether deliberately, self-deceptively, or by way of "elective affinity," or even "false consciousness"—anticipated the benefit and joined the containment effort in order to achieve that benefit. But the scenario puts a heavy strain on the idea of interest. We can easily imagine a campaign against self-interest that would erect barriers selectively so as to favor the interests of the campaigners, but the campaign that Ashworth describes does not display any obvious biases. The interest he imputes to the campaigners is that of avoiding social "collapse"—an interest not confined to any one class.[72] Even if an entrepreneurial class were to conclude that its self-interest required the erection of barriers against the pursuit of interest, would not other, non-entrepreneurial, classes reap the principal benefits? Instances like this, in which people can be said to be pursuing interest even as they oppose its pursuit, should at least alert us to the elasticity of the concept and point toward the double illusion under which much contemporary scholarship labors: that interest is the only thing that explains human behavior and that, once historians have linked acts with interests that might plausibly account for them, there is nothing left to explain.

Every link in the chain by which Ashworth tries to attach antislavery to class interest is open to objection. Consider first the provocation that is supposed to have set the entire process in motion—the emergence of a class of propertyless laborers, deprived of access to the means of production and forced to sell their labor power on the market. Ashworth is very eager that his readers take this to be the cutting edge of capitalism, rather than anything as general and abstract as "the market," partly, perhaps, because it has a robust Marxian flavor and partly because it permits him to claim that his explanation solves the supposedly knotty mystery of why the abolitionists "selectively" went to the aid of slaves rather than helping exploited workers generally: the problem of legitimating wage labor, he claims, was what they were really grappling with from the very beginning.[73] This claim might be plausible if the values that Ashworth describes offered any likely solution to the problems faced by a ruling class seeking to legitimate wage labor. We could understand why the rise of a new class of unruly laborers would inspire fear of crime or revolution or provoke anxiety about the future of republican institutions.

[71] Ashworth, "Relationship between Capitalism and Humanitarianism," 815 n.6.
[72] Ashworth, "Relationship between Capitalism and Humanitarianism," 822.
[73] Ashworth, "Relationship between Capitalism and Humanitarianism," 822.

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But why would it lead specifically to the veneration of family life, the exaltation of conscience, and a general preoccupation with the dangers of self-interest? These are reactions suitable to people whose eyes were fixed not only on the commodification of labor and the problem of controlling an underclass but on the broader problems created by the general breakdown of taboos on the pursuit of self-interest and consequent spread of competitive relations throughout society. "The market" is a term of art referring to this very phenomenon, and, when Ashworth actually addresses the task of explaining the middle-class preoccupation with self-interest, he repeatedly speaks of it as a response to "the spread of the market" rather than wage labor—apparently unaware that this undercuts his concluding exclamation that "it was precisely because of the spread of wage labor that they attacked slavery in the way they did."[74]

Even if we could agree that reformers' heightened concern for conscience and family stemmed from their anxieties about wage labor rather than the market, Ashworth has given us no adequate reason to think that these values were novel enough or determinate enough in their implications for action to account for the attack on slavery. He argues that the increasing number of wage-laborers, hitherto viewed as a slavish class unfit for citizenship in republican society, set in motion a massive shift of attitudes and practices. People searched for and found "new supports for individual morality and the social order" in three things: the family, the home, and individual conscience. "None had previously been ignored," Ashworth assures us, "but all came to be redefined." He goes on to say that, "in societies that had redefined them or were in the process of redefining them, slavery came to appear as a greater and greater evil. Thus a small but growing number of influential individuals were sensitized to the evils of slavery. As they viewed their own society differently, they initiated a new hostility to slavery (and other evils, too) and led the humanitarian movement."[75] Much hinges on just what this redefinition of family and conscience consisted of. Ashworth tells us only that the family lost its economic primacy as a center of production and that both family and conscience gained a new significance as counterweights to the growing force of self-interest. Exactly what it was about these surprisingly familiar and undramatic redefinitions that suddenly "sensitized" people to the evils of an institution that had been tolerated for centuries is none too clear. Ashworth merely asserts, without explaining, that "the heightened concern for the soul and the individual conscience, nourished within the family circle, *necessarily* intensified concern for slavery, which disrupted family ties and offered many temptations and opportunities for the slaveholder to sin" (my emphasis).[76] Evidently, we are expected to believe that, as middle-class people grappled with the problem of wage labor by

[74] Ashworth, "Relationship between Capitalism and Humanitarianism," 822–23, 828. The commodification of labor and the spread of "the market" are not two distinct things between which we must choose; the former is a particular case of the latter and one that it may be important to bring to the forefront in certain contexts. My usage follows that of Polanyi (for whom the countermovement was specifically aimed at containing "the market") and C. B. Macpherson, *The Political Theory of Possessive Individualism: Hobbes to Locke* (Oxford, 1964).

[75] Ashworth, "Relationship between Capitalism and Humanitarianism," 822.

[76] Ashworth, "Relationship between Capitalism and Humanitarianism," 827.

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appealing (never mind the mismatch between means and ends) to the values of disinterestedness, individual conscience, and domesticity, the growing disparity between their high ideals and the lowly life of the slave became so great that they could not stand it any longer.

This conclusion vastly overestimates the determinateness of values and the force of prescriptions. Values are prescriptions: when we value conscience, we embrace a prescription that tells us that the good life consists of attention to an inner voice, one that promises to bring us into conformity with the dictates of universal moral law. When we value hearth and home, we embrace a prescription that identifies the good life with the pleasures of the domestic sphere, as opposed to the cold world outside, and defines duty and fulfillment largely in terms of nurturing the next generation. Nothing in the values named by Ashworth specifies to whom these prescriptions apply; even the most ardent devotees of family and conscience need not feel any obligation to extend these blessings to slaves, or to neighbors, or to the residents of the next town, or to anyone beyond the circle of their closest associates.

Although these prescriptions regarding family and conscience were highly characteristic of middle-class people during the era of abolition, they were not distinctive. They were neither new at that time nor unique to that class. Conscience, in particular, has for centuries stood close to the heart of Christian teachings, and, if we were to look for a plateau when its importance was notably increased, the Reformation of the sixteenth century would be a better candidate than anything that happened in the eighteenth or nineteenth centuries. Family values, too, have had a long history. What redefinition of conscience and family in the eighteenth century could possibly account for the failure of earlier versions of these values, or any others, to provoke hostility to slavery? How could even redefined versions of open-ended values like these, which specify nothing inherently about their application, accomplish more toward the overthrow of slavery in a few decades than had been accomplished over a period of two millennia by the biblical injunction "Do unto others as you would have them do unto you?" The Golden Rule is the prescription of prescriptions, and it not only told people what the good life was but also to whom it should be extended. If that was not enough to inaugurate the history of active opposition to the institution of slavery, nothing in the realm of prescription could have done so.

Ashworth loses sight of the central questions. How are we to account for Davis's finding that practically no one before the eighteenth century saw in slavery anything worse than a necessary evil? Why at that time did people who were, by individual disposition, probably no more compassionate or scrupulous than their ancestors decide that an institution their ancestors had found tolerable was instead an intolerable evil and set about its destruction? I believe that they did this because of a shift of conventions, which prompted them to evaluate their responsiblity for suffering within an unprecedentedly broad arena and therefore to feel responsible for suffering that previous generations had perceived as inaccessibly remote. Ashworth can only suggest that their family-nurtured ideals of conscientious

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behavior rose to such stratospheric heights that they could no longer bear for slaves not to share in them.

It is especially ironic that Ashworth thinks that my scheme of explanation ignores changes in morality.[77] Here, Davis is closer to the mark. He sees that the most problematical and disturbing aspect of my explanation is precisely that it puts morality in motion, by giving it roots that run no deeper than convention. What Ashworth mistakes for a failure to recognize the historicity of morality is instead my insistence that prescription and practice not be naively lumped together, as if we could predict how people will act once we know what prescriptions they embrace. There is no one-to-one relation between prescription and practice; what people do is not merely an unfolding of the values they embrace. Of course, moral practices change: no one could write about the rise of antislavery or the emergence of the humanitarian sensibility who doubted this. Prescription also changes but not in any way that could explain the emergence of the humanitarian sensibility. To explain that development, we must take into account changes in the tacitly understood conventions that shape the way prescriptions are interpreted and reconciled (sometimes very circuitously) with practices. That is what my original essay strove to do, and that is what Ashworth's alternative leaves entirely out of account.

WAS THERE ANYTHING REVEALINGLY SELECTIVE ABOUT the decision of abolitionists to attack chattel slavery rather than labor exploitation generally? One might think that the question could be shelved in view of Davis's concession ("in theory," at least) that an abolitionist sincerely opposed to all forms of oppression might nonetheless have given priority to the slave trade and chattel slavery as "the most flagrant and remediable crimes against humanity."[78] What is there to explain when people choose to attack the most "flagrant" and "remediable" of the "crimes" before them, instead of doing something else? Problems that can be described in terms this strong naturally take priority. Davis has also acknowledged that "everyone knew that white workers were not really slaves." Since no one has ever thought that mistaken identity was an issue, I interpret this to mean that everyone also knew (taking both material and non-material conditions into account) that the plight of wageworkers—an extremely heterogeneous category—was not as severe as that of slaves. Davis has also allowed that the reformers, whatever their shortcomings, "have been vindicated by history: morally, they were right." Surely, history has also vindicated their sense of strategic priorities: as the *National Anti-Slavery Standard* remarked, "the first work of the reformers . . . is to establish universally the right of man to himself." The problem of slavery "underlies all

[77] Ashworth, "Relationship between Capitalism and Humanitarianism," 819.

[78] Davis, "Reflections on Abolitionism," 800. Davis believes that an abolitionist who thought like this would have left a written record of regrets about the tendency of attacks on slavery to obscure the abusive treatment of wage-laborers. I wonder. One common reaction to political opponents who try to cast doubts on one's motives is simply to ignore them, on the theory that base canards should not be dignified by a reply.

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other reforms . . . [for] there can be no universal reform . . . even no partial reform . . . in a nation that holds one-sixth of its people in bondage."[79]

These considerations suggest to me that, although there may be something puzzling about the abolitionists' attitudes toward wage labor and toward the poor generally, there is nothing at all puzzling about their selection of slavery as the problem of highest priority. Ashworth, however, laments my "retreat" from the "crucial" issue of "the selectivity of the abolitionists, their concern with chattel slavery rather than . . . wage slavery." Surprisingly, he concludes that on this point I have practically "nothing to say . . . Search Haskell's article as we may, we will find nothing that helps answer this question."[80]

Taken at face value, this is a most disheartening response, for it suggests that Ashworth has entirely missed the point of my essay, which from start to finish revolved around the problem of selectivity. The keystone of Davis's interpretation in *The Problem of Slavery in the Age of Revolution* was his claim that "as a social force, antislavery was a highly selective response to labor exploitation," one which—not unintentionally, but as a result of an unconscious hegemonic interest about which the reformers deceived themselves—"gave a certain moral insulation to economic activities less visibly dependent on human suffering and injustice."[81] Against this claim by Davis of an unconsciously intended selectivity on the part of the reformers, I have argued that, in a world that overflows with suffering, all humane acts are unavoidably selective. Because no possibility exists of acting unselectively, the fact that the reformers went to the aid of slaves rather than exploited wage-laborers cannot logically be taken to imply any sort of intention, conscious or otherwise, to "morally insulate" the exploitation of wage labor. To say (in the absence of more specific evidence) that the reformers unconsciously intended to perpetuate this or any of the multitude of other evils they left unrelieved is not essentially different from saying that a person who sends a contribution to Oxfam instead of Amnesty International displays an ominous indifference to torture—or that a person who makes the opposite choice thereby reveals a heartless disregard for hunger. The conclusion does not follow, except on the fallacious proposition that "if you are not for us, you are against us." As long as the question of selectivity is posed in hopes of inferring unconscious intentions, then, it is a question *mal posée*.

Ashworth, unlike Davis, rules out unconscious intentions from the start. This puts the question of selectivity on a different, and I think less vulnerable, footing, but the difference comes at a cost, and not all readers will think the cost acceptable.

[79] Davis, "Reflections on Abolitionism," 809, 810; John R. Commons, *et al.*, eds., *A Documentary History of American Industrial Society*, 10 vols. (New York, 1958), 7: 219. There has been no clear resolution of the immiseration controversy, but for purposes of argument I have assumed in everything that follows that no important difference existed between slaves and free workers at the level of the material conditions of life. No doubt some wage earners were as bad off as slaves, but the heterogeneity of the category "wage labor" makes it very likely that the group as a whole was better off materially than were slaves.

[80] Ashworth, "Relationship between Capitalism and Humanitarianism," 815–16. If selectivity were a problem at all, it would be as much a problem for Ashworth as anyone. Why would the same idealization of family and conscience that supposedly drove middle-class people to liberate slaves not also have driven them to ameliorate the lot of immiserated wageworkers?

[81] Davis, *Problem of Slavery in the Age of Revolution*, 251.

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Davis posed the question of selectivity in hopes of inferring unconscious intentions; Ashworth returns to it for the sake of highlighting what he takes to be the reformer's "false consciousness" or, more exactly, the incompleteness of their grasp of reality. Instead of assuming that the reformers knew in their hearts that wageworkers were victims of exploitation and that the reformers failed to render aid because they hid that knowledge from themselves, Ashworth assumes that reformers were blinded to the facts of exploitation by their class position. In Ashworth's view, it was the nature of their "involvement in society" that kept them from understanding the real conditions of working-class life, and he takes pains to stress that this is "not a matter of self-deception" but of class-induced ignorance.[82]

This explanatory strategy has the considerable virtue of making it unnecessary to postulate the existence of unconscious intentions (empirically undemonstrable, anyway), yet it retains the claim that the reformers' conduct was governed by their class position—which, at first glance, seems to be the same thing as saying that it was governed by class interest. Like any strategy, however, this one entails certain costs. By conceding that the reformers did not know, either consciously or unconsciously, that they should have gone to the aid of "wage slaves" as well as chattel slaves, Ashworth locates the hegemonic effects of antislavery agitation squarely in the category of unintended consequences. As such, neither the reformers nor anyone else bears responsibility for them. The reformers acted in ignorance, and, although their ignorance resulted from their privileged class position, it is not at all clear how any person or group could be thought blameworthy for failing to perform a duty of which, for whatever reason, they were genuinely ignorant. Ashworth's account has a blander tone than Davis's, and necessarily so, for, in the absence of intentionality and blameworthiness, words like "social control," "class oppression," and "hegemony" lose their bite.[83] It was presumably for this reason that Raymond Williams warned Marxists that, by withdrawing too far from the claim that there is a "process of determination," they risked emptying "of its essential content the original Marxist proposition . . . Intention, the notion of intention, restores the key question, or rather the key emphasis." In order for an interpretation to be called "Marxist," Williams believed it should at least depict the "organization and structure [of society] . . . as directly related to certain social intentions, intentions by which we define the society, intentions which in all our experience have been the rule of a particular class."[84]

Anyone hoping to defend Ashworth's strategy may have second thoughts at this point and want to reopen the question of just how "ignorant" the reformers were. Perhaps false consciousness produces a sort of ignorance over which people have some control; perhaps it is all a matter of degree; perhaps they could have

[82] Ashworth, "Relationship between Capitalism and Humanitarianism," 815.

[83] The paradoxical coexistence in the Marxian tradition of moralism with doctrines deeply subversive of the very idea of morality is the subject of Steven Lukes, *Marxism and Morality* (New York, 1985).

[84] Raymond Williams, "Base and Superstructure in Marxist Cultural Theory," *New Left Review*, 82 (1973): 7.

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overcome their class-induced blindness if only they had tried harder. But every step in this direction leads back toward self-deception or some functional equivalent. We mean, by self-deception, a form of ignorance that is self-inflicted and for which the self can be held partly responsible, on the theory that whatever the self can do, it has at least some power to undo. Any modification of false consciousness that would entitle Ashworth to say "they ought to have known better" would be vulnerable to all the same objections that we have already registered against self-deception.

We are now in a better position to appreciate the brilliance of Davis's formulation and see how much was at stake in it. By treating the hegemonic effects of antislavery as a result of intentions that were unconscious, Davis avoided the conspiratorial and reductionist overtones that plagued Eric Williams's interpretation of abolitionism, and yet Davis also retained a strong enough element of intentionality both to account causally for the reformer's comparative indifference to the problems of wage labor and to hold them at least ambiguously responsible for the consequences of that indifference. In contrast, Ashworth can account causally for their indifference—the nature of their "involvement in society" obscured from them the realities of the wage earner's situation—but he has no basis for imputing responsibility. Ashworth's account has victims but no villains, for he attributes the shortcomings of reform to the distortions of consciousness (something we "all possess to a greater or lesser degree") that come from our having to occupy a single vantage point in society, from which the whole can never be apprehended in its entirety.[85] The only villain is the human condition.

Ashworth's abandonment of intentionality may make his interpretation unacceptable in some quarters, but I have no objections on this point. I have argued all along that the hegemonic consequences of antislavery were unintended. The disagreement between Ashworth and myself lies elsewhere, in the reason for the abolitionists' comparative indifference to the suffering of wage earners and the poor. I have no doubt that their attitudes were shaped in part by their social position, but I am not persuaded that this is the main or the most useful explanation. I would distinguish, moreover, between class position and class interest, as two separate and possibly contradictory kinds of influence on perception and ideology. Ashworth lumps them together as if there could be no important difference between them: having accepted the idea that values grow smoothly out of interests, he naturally supposes that interests themselves grow out of class position in an equally regular fashion. There is no justification for these assumptions.

It is undoubtedly true that the reformers, because they came preponderantly from a single, affluent sector of society, had a limited experience of life and made errors when they generalized, as we all must, from their own experience to that of others in their society. Their vision was, then, influenced by their class position, just as Ashworth claims. But this is not at all the same thing as saying that their blind spots were induced by, or served, class interest. The wants, desires, and

[85] Ashworth, "Relationship between Capitalism and Humanitarianism," 815.

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passions that drive conduct no doubt take shape within the confines of the particular perspective that a person's social position affords, but they can originate anywhere. As Jon Elster has shown, there is no reason to assume that the interests shaped by position necessarily function to reinforce that position or even that the beliefs shaped by interest necessarily serve that interest. These are matters for empirical investigation, not sweeping generalization. Wishful thinking induced by interest, for example, can be fatal to the interest that induced it, and a taste for conspicuous consumption can fritter away the wealth that sustains the taste. The idea that a social situation always induces beliefs that are favorable to the ruling class or conducive to the perpetuation of the status quo is no more plausible than the idea that bad causes always give rise to bad effects, or *vice versa*.[86]

Is it possible, in spite of everything I have said to the contrary, that the comparative insensitivity of the abolitionists to the needs of wage labor was the result of class interest? Of course. Interest can never be ruled out. There is no human act so incontrovertibly disinterested that it cannot be construed as self-interested by enclosing it within a suitable interpretive framework.[87] But the ease with which any sophomore can perform this trick should make us skeptical of its value. Trying to explain events without referring to interests would be absurd, but we can take note of the infinite elasticity of the concept and the obsessiveness of our reliance on it and then develop a certain skepticism in the presence of those of every political persuasion who take it to be the sole proper terminus of explanation.

Let me close with a concrete example of selectivity, partly to bring these abstract issues into tighter focus and partly to satisfy Ashworth's curiosity about how I would explain the abolitionist's comparative unconcern with wage labor. The example is one my critics ought to find congenial, for it lends itself readily to an interpretation in terms of class interest. My aim is not to show that such an interpretation is flatly erroneous—no such demonstration could ever succeed, in my view—but rather to display in a specific case the tendency of explanation-by-interest to terminate inquiry prematurely.

The views of the American abolitionist Wendell Phillips are especially pertinent, because, in spite of his patrician origins, he ultimately came to believe something quite close to what Davis and Ashworth assume that all of the abolitionists would always have believed if their judgment had not been distorted by class interest. In 1871, when Phillips presided over a Labor Reform convention that declared war on the entire system of wage labor, alleging that it "enslaves" the working man and "demoralizes" and "cheats" both him and his employer, he construed the problems of labor in a way that is basically familiar to us today—that is, in a way that sees through the formal liberty of abused wageworkers and situates their problems on a continuum with those of chattel slaves, acknowledging differences in degree, perhaps, but not in kind.[88]

[86] Jon Elster, *Sour Grapes: Studies in the Subversion of Rationality* (Cambridge, 1983), 141–66. See also Robert W. Gordon, "Critical Legal Histories," *Stanford Law Review*, 36 (January 1984): 57–125.
[87] Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 1," 351 n.32.
[88] Wendell Phillips, *Speeches, Lectures, and Letters*, 2d ser. (Boston, 1894), 152.

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Phillips had not always thought that it made sense to compare wage earners and slaves, much less to lump their problems together as variations on the single theme of labor exploitation. Before the Civil War, like most humanitarian reformers of his generation, Phillips had found the very idea of "wages slavery" to be "utterly unintelligible," at least in the American context. In a remarkable statement in 1847, he acknowledged that manufactured articles were often the products of "unrequited labor" and that complaints against capital and monopoly were well justified in England and Europe, but things were different in America. Between the free laborers of the North and the slaves of the South, he drew a distinction so total as to stagger the imagination of any twentieth-century reader:

> Except in a few crowded cities and a few manufacturing towns, I believe the terms 'wages slavery' and 'white slavery' would be utterly unintelligible to an audience of laboring people, as applied to themselves. There are two prominent points which distinguish the laborers in this country from the slaves. First, the laborers, as a class, are neither wronged nor oppressed: and secondly, if they were, they possess ample power to defend themselves, by the exercise of their own acknowledged rights. Does legislation bear hard upon them? Their votes can alter it. Does capital wrong them? Economy will make them capitalists. Does the crowded competition of cities reduce their wages? They have only to stay at home, devoted to other pursuits, and soon diminished supply will bring the remedy . . . To economy, self-denial, temperance, education, and moral and religious character, the laboring class, and every other class in this country, must owe its elevation and improvement.[89]

These are obviously the words of a man who defined the self and its relation to the world in a manner that is extremely formalistic by present standards. Phillips seems not to have recognized any distinction between the form of freedom and its substance. He seems to have believed that a person who is physically and legally unconstrained is as free as any human being ever can be. Some of what he said resembles twentieth-century laissez-faire conservatism, but the resemblance is largely spurious, and even a reckless conservative today would be unlikely to assert in public, as Phillips unhesitatingly did, that workers need only practice "economy" in order to become capitalists in their own right, or that low wages can be raised by staying at home, or that self-denial, the ballot box, and good character form a sure path to advancement. These assertions are shot through with implications of a strongly formalist character, and everything turns on what we make of this alien element in Phillips's thinking.

The more alien an idea is, the easier it is to dismiss as ideology, but if we resist this temptation and take what Phillips said as an expression of the way the world once looked to an acute and certainly conscientious observer, we immediately discover that his attitude in 1847 toward wage labor is not at all perplexing—once we grant him his formalist premises. No matter how unrealistic we may think these premises, once adopted, they lead naturally and consistently to the conclusion that "wages slavery" is a contradiction in terms.

[89] Phillips quoted in John R. Commons, *A Documentary History of American Industrial Society*, 7: 220–21. See also Daniel T. Rodgers, *The Work Ethic in Industrial America, 1850–1920* (Chicago, 1978), 32; and Richard Stott, "British Immigrants and the American 'Work Ethic' in the Mid-Nineteenth Century," *Labor History*, 26 (Winter 1985): 86–102.

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The cast of Phillips's mind will not be unfamiliar to students of Victorian culture in England and America. He wrote as if a normal, healthy, adult self was an uncaused cause, a pure point of origin, a kind of cornucopia in which purposeful activity arose out of nothing and surged into the world. To be a person, in this view, was only incidentally to be a material body, caught up in nature's endless fabric of cause-and-effect relations and fully subject to all the natural processes of growth, disease, and decay. The heart of personhood lay in the will, and the will, though not necessarily supernatural, was uncanny. The self formed a pure point of origin through its mysterious capacity of "willing," of almost magically transmuting the evanescent, inward, and private experience of desire and choice into the concrete, outward, and public phenomenon of action. More mysterious still, the resulting external acts produced consequences that corresponded with, or satisfied, the internal desires—or, at least, could do so if the will allowed itself to be guided by the dictates of reason and knowledge.

The self's ability to trigger causal chains and set events in motion meant that it was very much *in* the world, and yet, because it was always the producer, never merely the product of any antecedent chain of natural causation, it was not quite *of* this world. Every merely natural cause is a transitive link in a chain and can be construed both as cause of what it produces and the effect of some link antecedent to it—which, in turn, can be seen as the effect of some cause still more remote, and so on indefinitely. But the self in the act of willing was seen as a new, non-transitive, causal beginning, independent of everything but the First Cause and His providential order—a very special sort of dependence that even predestinarians insisted did not dilute in the least the self's responsibility for whatever it willingly did.[90] In the world of formalism, the self's every act was deemed voluntary insofar as will was in it, and scarcely anything other than direct physical coercion was thought capable of displacing the will and emptying an act of its voluntary character.

Formalism assigned such a high level of autonomy to the will that the context within which the will formed faded into the distant background, making nearly all deliberate acts appear voluntary. Circumstances, the stuff of environmentalist interpretations of behavior, counted for very little in judging what the self was responsible for. The self was responsible for whatever its activity led to, almost without regard to the circumstances surrounding choice and action. The formalist had no difficulty in recognizing that the will actively assessed its environment and inclined toward one act rather than another *in light of* various circumstantial factors, but to assign these factors causal significance and treat them as the explanation of what the self did would have been to deprive the self of the uncaused character that made it what it was. The cause (and therefore the explanation) of what people did was their choosing to do it, not the circumstances surrounding the choice. We of the twentieth century impute to circumstances a

[90] "Man is entirely, perfectly and unspeakably different from a mere machine, in that he has reason and understanding, and has a faculty of will, and so is capable of volition and choice; . . . so that he has liberty to act according to his choice, and to do what he pleases . . . [and so is fully worthy of] praise . . . [or] punishment"; Jonathan Edwards, *Freedom of the Will* (New Haven, Conn., 1957), 370.

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power to mold and induce choice—virtually to cause it and certainly to explain it—that mere circumstances could not possess in a formalist's eyes.

These are the conventional premises defining selfhood and governing the allocation of praise, blame, and responsibility that I believe Phillips and most members of his generation shared. They account for what we perceive as his curious insensitivity to the plight of exploited wageworkers and the poor. As seen from our (not necessarily superior) vantage point, Phillips and his contemporaries were blind to an entire range of environmental factors that powerfully influence conduct yet leave people formally free to do as they please. Where we see a deceptive form of freedom, lacking substance, they saw a large and sunlit realm of authentically voluntary choice.

To a person who sees the world as Phillips did in 1847, the difference between a free worker and a slave is not a matter of degree but of quality. Phillips imputed to free workers—a class "neither wronged nor oppressed" and equipped with "ample power to defend themselves"—a degree of autonomy so great that their problems were not commensurable with those of slaves. Free workers, unlike slaves, were masters of their own fate. Even by our twentieth-century standards, there is an irreducible element of voluntariness in even the most fated of contractual arrangements: the free worker can, at a cost, always say "no" and refuse the employment contract. For a formalist, the perceived cost approximates zero because the conditions impelling the employee to accept undesirable conditions of work are assigned little weight.

Because wage earners were thought free to rise or fall to whatever social position corresponded to their inner merit, those who found themselves in a state of misery had little besides themselves to blame. Formalist assumptions did not prevent decent people like Phillips from feeling passive sympathy for a pauper or playing the Good Samaritan in particular cases by "lending a hand" to a person "down on his luck." But there could be no general attack on the institution of wage labor or any systematic attempt to reform the institution (indeed, it could not even be perceived in a causal role), as long as reformers saw the world through lenses that made most of the poor appear to be deeply complicitous in their own suffering. Because formalists perceived the causes of suffering to lie largely in the victims' own apparently unimpeded choices (ultimately, in defects of the will), the only general remedy for their misfortunes was to help them perfect the arts of self-mastery and educate them about the predictable consequences of their actions. The lessons of "economy," "self-denial," "temperance," and "education" thus stood at the head of Phillips's list of priorities. We must struggle to appreciate that, to him and to most members of his generation, this thin gruel was not only an adequate prescription for social improvement but a trenchant identification of what on formalist premises were the vital nutrients of progressive change.

Although, on formalist assumptions, immiserated wage earners were the cause of their own misery, slaves could not possibly be cast in the same causal role.[91] The

[91] Here I may understate the extravagance (in our eyes) of the formalist concept of voluntariness, for at a more abstract level the question of whether people could ever be thought to have voluntarily

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status of chattel property shrinks to the vanishing point every hint of voluntariness, making the slave the perfect victim, a person whose misery even in formalist eyes is untainted by any suspicion of complicity. It is for this reason that the concern for slavery displayed by Phillips's generation of reformers could be more wholehearted and vehement than their reaction to most other forms of suffering. In the eyes of an observer who makes little or no distinction between formal and substantive freedom, legally free workers bear a large measure of responsibility for their own plight, but the suffering of slaves, being wholly involuntary, is the responsibility of everyone who has any power to stop it.

We can see, then, that the different treatment Phillips accorded to slaves and wage laborers did not result from a discrete bias against wage laborers but from a system of conventional assumptions that colored his and his generation's thinking about all human conduct. This is not the place to sort out the connections between this set of assumptions and the rise of capitalism, or between those two developments and the great upheaval in the conventions governing causal attribution that made reformers of formalists like Phillips (by expanding causal horizons and thereby overcoming the chilling effect of their extravagant concept of voluntariness) and then helped shatter formalism itself. It is indeed true that formalism, like opposition to slavery, "cannot be divorced from the vast economic changes" of the eighteenth and nineteenth centuries, but neither can it be understood as a reflex, however unconscious or indirect, of those changes.[92] No doubt it insulated entrepreneurs against the barbs of conscience and public opinion as they drove hard bargains with workers too poor to "stay at home," waiting for wages to rise. But it had many effects that were not at all congruent with the needs of capitalism and may even have obstructed its path.

The formalist cast of mind was not something capitalism brought into existence. Six hundred years before Phillips was born, Thomas Aquinas defined the outer perimeter of voluntary action no less extravagantly than Phillips did. In an image that Hobbes would borrow four hundred years later, and that Aristotle had employed more ambivalently a millennium and a half earlier, Aquinas, the great codifier of Christian doctrine, insisted that even a man at sea, who chooses to throw his possessions overboard rather than risk death in a storm, is acting voluntarily. "That which is done through fear is voluntary," said Aquinas. "The will cannot be compelled to act . . . violence cannot be done to the will."[93]

renounced their freedom and chosen to become slaves was taken very seriously by several generations of rights theorists, beginning in the early sixteenth century. At a more prosaic level, one of the principal rationales for the legitimacy of slaveowning construed the slave as originally a captive in war, who might well choose slavery over death. See Richard Tuck, *Natural Rights Theories: Their Origin and Development* (Cambridge, 1979), 3, 49, 54, 100; and Davis, *Problem of Slavery in Western Culture*, 117–20.

[92] Davis, "Reflections on Abolitionism," 806.

[93] Aquinas and Aristotle, unlike Hobbes, recognized that the seafarer's act is of a "mixed kind" and that there is a sense in which it is involuntary. But both assigned clear priority to the sense in which it is voluntary, just as Hobbes did. *Basic Writings of St. Thomas Aquinas*, Anton C. Pegis, ed., 2 vols. (New York, 1945), 2: 234, 231; *The Nichomachean Ethics of Aristotle*, D. P. Chase, trans. (New York, 1911), 44–45 [Book III]; Thomas Hobbes in *British Moralists, 1650–1800*, D. D. Raphael, ed., 2 vols. (Oxford, 1969), 1: 55 [*Leviathan*, chap. 22].

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We sense how truly alien to our own experience the perspective of formalism is when we see how literally Aquinas believed that the willing self was a new causal beginning, a pure point of origin. Turning a radically blind eye to all the environmental factors that (in our eyes, at least) shape the will and tailor life's opportunities to the imperatives of a particular social order—to all the factors, in other words, that create a gap between the form and the substance of freedom—Aquinas denied that there was anything at all outside the self, anywhere in God's created universe, that could move the will, other than its own object (the "apprehended good") and the gentle art of persuasion.

> Now it is a law of providence that everything is moved by its proximate cause . . . But the proximate moving cause of the will is the apprehended good, which is its object, and the will is moved by it as sight is by color. Therefore no created substance can move the will except by means of the apprehended good—in so far, namely, as it shows that a particular thing is good to do; and this is *to persuade*. Therefore no created substance can act on the will, or cause our choice, except by way of persuasion.[94]

We are the heirs today of a revolt against formalism that bore fruit in the years 1880–1920 and inaugurated the modern era in social thought.[95] To expect people who lived prior to this intellectual watershed to share our antiformalist concept of freedom and responsibility is to project into an age importantly different from ours the deceptively familiar features of our own world. It is also to underestimate the genuine historical novelty of antiformalism, one prophetic stream of which first struggled into existence in the thought of Owen, Fourier, Marx, Engels, and a cadre of others, who were the first to criticize the highly abstract and autonomous characterization of the self and its relation to the world that came to be known in the nineteenth century as individualism. To find that the abolitionists defined freedom formalistically and were therefore more sympathetic to slaves (whose lack of independence made them perfect victims) than to impoverished wageworkers (whose formally defined freedom meant that they always bore some responsibility for their own misery) is merely to reaffirm that the likes of Owen and Marx were truly harbingers of a new age who construed the world in ways that were profoundly unconventional, which is to say, culturally unavailable to most people of their era.

Given the antiquity of formalism and its centrality in the Christian tradition, the question we need to ask about Wendell Phillips is not why he "selectively" attacked slavery and ignored the plight of wage-laborers in 1847, but why, a few decades later, he and many others abandoned the premises of formalism and adopted a far less robust concept of the self that put the entire problem of responsibility and reform in a dramatically new light. As long as we are mesmerized by the idea of interest, this question cannot even come up.

[94] *Basic Writings of St. Thomas Aquinas*, 2: 168–69 [Summa Contra Gentiles, Book III, chap. 88].

[95] Morton White, *Social Thought in America: The Revolt against Formalism*, expanded edn. (Boston, 1957); H. Stuart Hughes, *Consciousness and Society: The Reorientation of European Social Thought, 1890–1930* (New York, 1958); Talcott Parsons, *The Structure of Social Action: A Study in Social Theory with Reference to a Group of Recent European Writers* (New York, 1937); James T. Kloppenberg, *Uncertain Victory: Social Democracy and Progressivism in European and American Thought, 1870–1920* (New York, 1986); Haskell, *Emergence of Professional Social Science*.

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If we asked Davis and Ashworth why Phillips changed his mind between 1847 and 1871, we would likely receive an answer that treats twentieth-century antiformalist assumptions not as anything mediated by convention or emergent in history but simply as reality. Davis's answer would presumably explain how Phillips managed to stop deceiving himself about formalistic assumptions that, unconsciously, he had always known were unrealistic. Ashworth—perhaps after hesitating momentarily at the thought of a change of values without any corresponding change in the interests they supposedly grew from—would presumably strive to show how Phillips managed to break the grip of "false consciousness" and began seeing things in their true light. From both vantage points, Phillips's initial opinion requires explanation, since it was an error, but his later opinion would not seem to merit an explanation because, when people give up error for truth, there is nothing to explain. In contrast, if we take the role of conventions seriously, put interest to one side, and do not press the all-too-human claim that our own views correspond with reality, Phillips's opinion in 1871 can be seen as part of a cultural transformation of far-reaching significance, one that we cannot hope to understand until we have charted the entire course of that upheaval in the conventions defining selfhood and governing the allocation of praise, blame, and responsibility that began in the eighteenth century.

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Review Article

Intellectual History after the Linguistic Turn: The Autonomy of Meaning and the Irreducibility of Experience

JOHN E. TOEWS

IN AN ARTICLE PUBLISHED IN 1981 that outlined the current trends and future prospects of intellectual history, William J. Bowsma coupled a pessimistic assessment of the "obvious and probably irreversible" professional decline of the specialized "history of ideas" with an optimistic projection of the transformation of intellectual history into a pace-setting participant in a more broadly conceived "history of meaning." Bowsma counseled intellectual historians to resist debilitating defensive anxieties regarding their professional identities and the autonomy of their field and to recognize that the "remnant chiefly worth saving" from their traditional concerns—the focus on the production, reproduction, and transmission of meanings in various historical periods and cultural contexts—placed them at the center of the most interesting and innovative work currently being produced, not only by their fellow historians but more generally in the humanities and social sciences. The apparent decline of intellectual history as an academic subdiscipline might be construed as a sign of its maturation and assimilation into the general current of historically oriented studies of human culture: "We no longer need intellectual history because we have all become intellectual historians."[1]

Bowsma premised his assessment of intellectual history's possible fate in the 1980s on two general assumptions. First, he connected the decline of the traditional "history of ideas" to the historical disintegration of the belief that conscious, rational thought was the universal, species-defining, and thus "highest" form of human activity, determining the criteria of truth in matters of both fact and value. By viewing the works produced by intellectual elites claiming this universal perspective as merely a subgroup within the larger category of cultural constructions of meaning that were "a function of the human organism as a whole," intellectual historians were finally coming to terms with their own persistent

[1] William J. Bowsma, "Intellectual History in the 1980s: From History of Ideas to History of Meaning," *Journal of Interdisciplinary History*, 12 (Autumn 1981): 279, 283, 280.

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The books under review are (in alphabetical order):

Jean-Christophe Agnew, **Worlds Apart: The Market and the Theater in Anglo-American Thought, 1550–1750** (Cambridge, 1986);

David A. Hollinger, **In the American Province: Studies in the History and Historiography of Ideas** (Bloomington, Ind., 1985);

Martin Jay, **Marxism and Totality: The Adventures of a Concept from Lukács to Habermas** (Berkeley, Calif., 1984);

Peter Jelavich, **Munich and Theatrical Modernism: Politics, Playwriting and Performance, 1890–1914** (Cambridge, Mass., 1985);

Modern European Intellectual History: Reappraisals and New Perspectives, Dominick LaCapra and Steven L. Kaplan, eds. (Ithaca, N.Y., 1982);

Dominick LaCapra, **Rethinking Intellectual History: Texts, Contexts, Language** (Ithaca, N.Y., 1983);

Dominick LaCapra, **History and Criticism** (Ithaca, N.Y., 1985);

Allan Megill, **Prophets of Extremity: Nietzsche, Heidegger, Foucault, Derrida** (Berkeley, Calif., 1985);

J. G. A. Pocock, **Virtue, Commerce and History: Essays on Political Thought and History, Chiefly in the Eighteenth Century** (Cambridge, 1985);

Mark Poster, **Foucault, Marxism and History: Mode of Production versus Mode of Information** (Cambridge, 1984);

Philosophy in History, Richard Rorty, J. B. Schneewind and Quentin Skinner, eds. (Cambridge, 1984);

The Return of Grand Theory in the Human Sciences, Quentin Skinner, ed. (Cambridge, 1985).

practice of historicizing and contextualizing the intellect and its products. Second, Bowsma assumed that the integration of intellectual history into the history of meaning would not simply invert the hierarchies of the past, giving privileged status to bodily needs, psychological cravings, and social interests, rather than to conscious ideas or rational argument, but would avoid reductionist procedures altogether. The new categories of "meaning" and "experience" would not replicate the old polarities of thought and reality, consciousness and being, but would encourage an integrated concept of historical reality as meaningful experience in which "the creative interpretation of experience also shapes experience" and "some sense of meaning is . . . both a condition and a product of experience."[2]

This new agenda for intellectual history had a number of significant implications. It promised an end to isolating specialization and the resulting disciplinary and subdisciplinary feuds and anxieties. Intellectual historians could feel encouraged by the affirmation of the centrality of their concerns and the relevance of their professional skills for the critical questions animating current scholarship. More specifically, Bowsma envisioned an increasingly self-conscious search for inter-

[2] Bowsma, "Intellectual History in the 1980s," 283, 288.

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pretive strategies to reconstruct the production and transmission of meanings at various cultural levels and in different cultural contexts. This search would encourage less unilateral and parasitic, more intimate and dialogical, relationships with sister disciplines such as cultural anthropology, art history, and philosophy and a more systematic use of linguistic models and procedures, since the primary medium of meaning was obviously language. The cultural world, the realm of meaningful experience, he claimed, might be described as a "vast rhetorical production." Entering into the mainstream of the history of meaning would involve the intellectual historian in a linguistic turn.[3]

The dozen recently published books under review in this essay are too narrow and arbitrary a sampling (skewed both toward modern European intellectual history and the analysis of the artifacts of intellectual elites) to provide anything like an adequate test of Bowsma's projections. It is striking the extent to which they virtually all assume, however, the validity of his general claim that intellectual history is an integral part of the interdisciplinary study of the history of meaning and that the pursuit of this study involves a focused concern on the ways meaning is constituted in and through language. A new self-confidence is clearly evident. The fears of being conquered and colonized by the perspectives and methods of social historians, so prevalent among intellectual historians just a few years ago, have diminished considerably, and one can even find warnings about the dangers of overconfidence and intellectual imperialism.[4] But the interdisciplinary and intradisciplinary tensions and disputes have not been resolved or put aside. Although a reconsideration and perhaps rearrangement of conventional categories and oppositions is clearly under way, the first result of this self-reflective activity appears to be a displacement of conventional distinctions and disputes into the new terms of meaning and language. We may all be historians of cultural meaning as constituted in language, but we still investigate meaning at different cultural levels involving different kinds of texts requiring different kinds of linguistic analysis. As Martin Jay and others have correctly pointed out, the question of whether or not intellectual history should take a linguistic turn involves the preliminary question of which among a variety of linguistic theories of meaning a historian should choose.[5] The linguistic turns chosen often overlap with traditional divisions between historians of sociocultural structures, political ideologies, and the artifacts of "high" culture, as well as between historians of the different national cultures in which various theories have been formulated and elaborated.

In spite of these indications of continuing disagreement along familiar lines, one can also discern a new framework of questions, or a "problematic," around which the discussions among intellectual historians are increasingly organized. Most seem ready to concede that language can no longer be construed as simply a medium, relatively or potentially transparent, for the representation or expression

[3] Bowsma, "Intellectual History in the 1980s," 290.

[4] See, for example, Hollinger, *In the American Province*, 188.

[5] Martin Jay, "Should Intellectual History Take a Linguistic Turn? Reflections on the Habermas-Gadamer Debate," in LaCapra and Kaplan, *Modern European Intellectual History*, 87.

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of a reality outside of itself and are willing to entertain seriously some form of semiological theory in which language is conceived of as a self-contained system of "signs" whose meanings are determined by their relations to each other, rather than by their relation to some "transcendental" or extralinguistic object or subject. Disputes arise from a concern for the implications of a commitment to a semiological theory of meaning in its extreme form. Such a commitment would seem to imply that language not only shapes experienced reality but constitutes it, that different languages create different, discontinuous, and incommensurable worlds, that the creation of meaning is impersonal, operating "behind the backs" of language users whose linguistic actions can merely exemplify the rules and procedures of languages they inhabit but do not control, that all specialized language usages in a culture (scientific, poetic, philosophical, historical) are similarly determined by and constitutive of their putative objects. Within this perspective, historiography would be reduced to a subsystem of linguistic signs constituting its object, "the past," according to the rules pertaining in the "prisonhouse of language" inhabited by the historian. As Keith Baker warns, structuralist theories of language seem to extend "an offer of the entire world as a domain of meaning, but at the cost of our historical souls."[6] Although expressions of apocalyptic fear of the end of history as we have known it or millenarian hopes for a totally new kind of history can occasionally be discerned in the current literature, the predominant tendency is to adapt traditional historical concerns for extralinguistic origins and reference to the semiological challenge, to reaffirm in new ways that, in spite of the relative autonomy of cultural meanings, human subjects still make and remake the worlds of meaning in which they are suspended, and to insist that these worlds are not creations *ex nihilo* but responses to, and shapings of, changing worlds of experience ultimately irreducible to the linguistic forms in which they appear. Insofar as it is a type of history, intellectual history cannot be completely identified with a radical hermeneutics that assumes nothing exists beyond meanings, but it must address the issue of explanation, of why certain meanings arise, persist, and collapse at particular times and in specific sociocultural situations. Although no easily discernible, common position emerges in the works under review, they can be seen as participating in a common discourse in the sense that they address themselves to the promises and the problems of sustaining the dialectical unity of and difference between meaning and experience (as all historians must) in the wake of the linguistic turn.

If the general field of historical studies could be redefined as the investigation of the contextually situated production and transmission of meaning, intradisciplinary turf battles between social and intellectual historians would appear to lose their point. The disjunction between external event and internal idea, between objective process or structure and subjective representation or expression, would dissolve if a consensus could be established that the plowing of

[6] Keith Michael Baker, "On the Problem of the Ideological Origins of the French Revolution," in LaCapra and Kaplan, *Modern European Intellectual History*, 200.

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a field, the abuse of a child, or the storming of a fortress were as much contextually situated, meaningful social actions as the construction of a philosophical argument, the choice of a metaphor, the publication of a journal, or the performance of an opera. However, a number of the most persuasively argued contributions to contemporary debates on the future of intellectual history indicate that, even if historians could agree that they were all engaged in reconstructing the history of meaning, intradisciplinary tensions and conflicts would persist. The essays of Roger Chartier and Dominick LaCapra address this issue critically and directly, focusing on the hegemonial claims of "sociocultural history" or the "social history of ideas" within the history of meaning.[7]

What both Chartier and LaCapra find objectionable in the sociocultural history inspired by the French *Annales* school is the ways in which this history objectifies meaning in order to open up the sphere of cultural history to the methodological procedures and organizing categories developed in socioeconomic analysis. Such objectification, they argue, has taken two major forms. It is characterized, first of all, by an overwhelming focus on the reconstruction of impersonal, collective, "unconscious" structures of perception, feeling, and thought, on the inventory of available tools or terms (the lexicon) and the rules and procedures of their use (the syntax) that together make up the collective *mentalité* of a period or culture. Particular acts of meaning-production are reduced to instances of the underlying structures that enable or restrict their possibility. The connection to Michel Foucault's concept of linguistic performances as instances of rules contained in archaeologically recoverable "archives" or "epistemes" or "discourses" is obvious, although Foucault eschewed the totalizing ambitions and causal claims of many sociocultural historians. The second way meanings have been objectified in the social history of ideas is through defining individual meanings as self-identical ideas or "essences" that can be extracted from their textual contexts and treated as commodities distributed and consumed in statistically measurable patterns. Although neither Chartier nor LaCapra denies the importance of reconstructing linguistic codes or of analyzing the production and distribution of meanings as commodities, they both perceive the history of meaning as a complex process of linguistic creativity and communicative action that is irreducible to socioeconomic models and demands the interpretive skills intellectual historians and literary scholars have developed in the critical reading of the artifacts of "high" culture.

Chartier and LaCapra each propose a sweeping revision of the categories imposed on the history of meaning by sociocultural historians. First, they question the binary opposition of elite and popular culture, especially in the form that implies that the popular is more basic and inclusive. Such a categorical opposition constitutes a gross oversimplification of the heterogeneous strands of cultural meaning present within any historical culture and obscures the complexity of the relations between them. The category of elite culture hides significant distinctions

[7] The following discussion draws on LaCapra's essays in both *Rethinking Intellectual History* and *History and Criticism*. Roger Chartier, "Intellectual History or Sociocultural History? The French Trajectories," in LaCapra and Kaplan, *Modern European Intellectual History*.

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between the "high" culture articulated in the complex, self-consciously constructed artifacts of philosophers, theologians, artists, and other "intellectuals," the "official" culture supported or propagated by state authorities or established churches, and the "dominant" or "hegemonial" culture of social and political elites. Similarly, "popular" culture must be conceptually differentiated into "general" or "common" culture, the culture of socially dominated or politically excluded groups like peasants or wage-laborers, the "everyday" culture of work, play, family, and gender relations in which all groups shared in specific ways, the "political" or "public" culture in which citizens engage in debate on their common and conflicting goals as members of an organized community, and the "mass" culture of the consumers and producers of commodified meanings.[8] As Chartier insists, although each cultural level can be reconstructed as a world of meanings, these heterogeneous worlds are not autonomous but appear "as compound worlds that bring together, in a virtually inextricable mixture, elements of very diverse origins."[9] The multilayered complexity of a culture composed of such heterogeneous compounds undermines the assumption of distinct cultural "objects" demanding different methods of analysis. Both interpretive and reconstructive techniques are needed for a nonreductive investigation of the history of meanings at all levels. Anonymously structured codes enter into the most individualized, self-conscious, and linguistically refined artifacts of "high" culture, just as the religious beliefs of a common miller should not be reduced to a simple instance or determination of a reified, "popular" *mentalité*.[10]

Second, deconstructing the opposition of elite and popular entails a reconsideration of the oppositions of creation-reception and production-consumption as well. According to Chartier, reception and consumption should be viewed as creative, productive actions. The appropriation of imposed or distributed meanings, even in the most authoritarian and closed homogeneous cultures, is not simply a passive assimilation but is characterized by interpretive activity that involves resistance and evasion as well as substractive, supplemental, and transformative revisions. Meanings are never simply inscribed on the minds and bodies of those to whom they are directed or on whom they are "imposed" but are always reinscribed in the act of reception.[11] LaCapra develops this point into a general theory, often formulated in the terminology of Derrida, about the reading and interpretation of texts. Reading, viewing, or listening are construed as critical, creative actions in which the dynamic, multidimensional aspects of the production of meaning in the original artifact or text are constantly questioned, displaced as repetitions in different systems of meaning, supplemented, parodied, and reinscribed in various other ways in a process that can never attain the finality of definitive, fixed interpretation but that involves a continuous play and prolifer-

[8] LaCapra, *History and Criticism*, 74–78.
[9] Chartier, "Intellectual History or Sociocultural History," 34.
[10] See LaCapra's extended review of Carlo Ginzburg's *The Cheese and the Worms* in *History and Criticism*, 45–69.
[11] Chartier, "Intellectual History or Sociocultural History," 37–38.

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ation of meanings.[12] While Chartier sees the dissolution of the dichotomy between producer and consumer as a premise for erasing the methodological divisions between sociocultural and intellectual history, LaCapra uses the concept of creative consumption as a means of reinstating the "relative specificity" of intellectual history in both an intra- and interdisciplinary context, as the form of historical investigation that is especially attentive to the sophisticated art of textual reading and that takes as its privileged objects those "great" texts in which the active, self-questioning, critical, and constructive aspects of the process of meaning production are most clearly evident. Intellectual historians are especially concerned with those historical artifacts that are "good for reading" or "good for thinking," whose multilayered and conflicting levels of meaning demand critical engagement and dialogue.[13]

The third major categorical opposition questioned by Chartier and LaCapra is that between representation and reality. The conventional procedures of sociocultural history are perceived as implying that meanings are not produced as actual, "material" events within the dense interrelations of systems of signs but are epiphenomenal and determined by an extralinguistic reality through relations of cause, reflection, representation, analogy, or expression. The new, semiologically oriented history of meanings, however, begins with the assumption that meanings do not simply mirror or represent but actually constitute or create the reality experienced by human beings. The "experience" that generates the revising and transforming procedures of creative consumption is never "raw" but "always already" constituted in meaning. Two consequences flow from this premise. First, all forms of historical evidence have not only a referential, "documentary" dimension insofar as they refer to various historical forms and contents that exist as already constituted, "given" meaningful experiences, but they also exhibit a "work-like" dimension as acts of meaning-production in which the given forms and contents are set into new patterns of relationship in order to constitute a new meaningful reality. This duality is present in parish registers and trial reports, philosophical treatises and musical compositions. In this case as well, however, LaCapra goes beyond Chartier in claiming that different kinds of texts embody the duality in different ways and that the relative specificity of intellectual history is tied to its interest in understanding those texts that are least documentary and most work-like. LaCapra is concerned that the intricate internal dynamics by which meaning is constituted in great texts will be ignored by historians taught to "gut" texts for information about an extratextual reality. Conventional methods of paraphrase and synopsis, for example, imply a belief that texts are not events in the history of language in which the process of meaning-construction takes place but simply containers of ideas or facts that can be extracted from them and then

[12] LaCapra, *Rethinking Intellectual History*, 23–69.

[13] The allusion is to Claude Lévi-Strauss's comment in *The Savage Mind* that certain things can be "good to think" just as others might be good to eat.

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set into relation with other similarly mined ideas or facts.[14]

A second implication of the deconstruction of the distinction between representation and reality is the need to reconceptualize the issue of text and context. First, the context in which a textual artifact or systematic pattern of signification is to be understood must itself be conceived as a compound world of constituted meanings, as a text requiring interpretation. Both text and context are complex relations of "signifying practices."[15] The connections between them must thus be construed as "intertextual." The context never "explains" the text in the sense of providing the essence of its appearance or the cause of its effect or the reality of its representation. LaCapra expends a great deal of energy in the exposure and systematic critique of tendencies toward the reductive contextual interpretation of textual events. His critical reviews of Carl Schorske's *Fin de siècle Vienna* or Stephen Toulmin and Allan Janik's *Wittgenstein's Vienna*, for example, continually return to the two primary ways in which intellectual history may fall prey, even in the practice of its most distinguished exemplars, to the sociocultural historian's reading of texts as documentary representations of reality rather than complex textual events in a textualized context.[16] Restrictive documentary readings that paraphrase the complex workings of a text in terms of essential contents are connected to equally restrictive readings of the context as a matrix for the production of those contents or as the source of the realities reflected in them. Reading LaCapra's critical commentaries, one begins to wonder if it is possible to avoid the pitfalls of a referential or representational theory at all without ceasing to "do" history and restricting oneself to thinking about it. Has the theory of the linguistic density and complexity of texts, contexts, and their apparently circular relationships outrun its possible utility as either a clarification of, or guide for, historiographical practice? An instructive commentary on such questions is provided by two exceptionally talented young historical scholars, who have written interesting first books devoted to historical analysis of the high culture of dramatic writing and theatrical production in relation to other levels of cultural meaning and to shifting historical experiences: Peter Jelavich's *Munich and Theatrical Modernism: Politics, Playwriting and Performance, 1890–19141* and Jean-Christophe Agnew's *Worlds Apart: The Market and the Theatre in Anglo-American Thought, 1550–1750*.

Jelavich tends to eschew self-reflective or programmatic theorizing and can in many ways be read within the framework of conventional intellectual history, especially as practiced by his unconventionally brilliant mentor, Carl Schorske. The backbone of Jelavich's study consists of paraphrases and synoptic reconstructions of a number of texts in a particular genre produced in a particular time and place. The homologies in content and form as well as the temporally situated patterns of change among these artifacts of "high" culture are in turn interpreted

[14] LaCapra, *Rethinking Intellectual History*, 33–34; see also Hans Kellner, "Triangular Anxieties: The Present State of European Intellectual History," in LaCapra and Kaplan, *Modern European Intellectual History*, 116–17.

[15] LaCapra, *Rethinking Intellectual History*, 26–27.

[16] LaCapra, *Rethinking Intellectual History*, 84–117; LaCapra, *History and Criticism*, 81–86.

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and explained by placing them in a series of contextual relationships. The "point" of theatrical modernism is to be grasped through a reconstruction of its historical contexts, both contexts of meaning and contexts of experience. Modernism, he suggests, "was the product of a game played with diverse cultural pieces on a political, social, and commercial terrain that was constantly shifting."[17] Like Schorske, Jelavich sees the shifts in the political terrain, specifically the external defeat and internal collapse of liberal politics, as the ultimate context or as the context of last resort for understanding the fate of modernist theater. His book begins with a description of the crisis of Bavarian liberalism in the late nineteenth century and concludes with a description of its accelerating collapse during and after World War I.

Despite such indications of the persistence of a traditional historiographical standpoint, Jelavich's work is clearly sensitive to the kinds of concerns and perspectives voiced by LaCapra and Chartier. This awareness is evident first of all in the choice and definition of his "object" of investigation. Modernism is defined primarily in terms of the emergence of a nonmimetic, nonrepresentational concept of language as a system of signs within which worlds are constructed rather than reflected. This concept of language is seen as having critical and deconstructive as well as utopian and constructive potentialities to reformulate the function of art in society. This concept made possible a critical dismantling of inherited cultural languages as "ideological" constructions and justified the belief that the aesthetic creation of new patterns of meaning, especially in the theater, could also produce new worlds, construct new modes of individual existence, and forge new kinds of communities.

Second, Jelavich's own reconstruction of the culture of theatrical modernism and its various cultural contexts tends to treat these patterns of meaning as heterogeneous and compound worlds. The languages of theatrical modernism expropriate and transform signifying elements from subterranean, oppositional traditions in official academic neoclassical culture, dominant Catholic religious culture, traditional folk culture, and commercialized mass culture. The modernist culture that emerges as these elements are put into experimental relationships creating new patterns of meaning and a new self-consciousness about the ways in which meaning-construction occurs is in turn situated by the historian in relationships with other compound and heterogeneous cultural worlds that together compose what we might call the cultural universe of *fin de siècle* Munich.

But Jelavich is not content to summarize or paraphrase the process of meaning-construction in a number of texts situated within, and interacting with, multiple cultural contexts, nor would he, I think, be willing to define his own history as simply another moment in the "game" of deconstructing and constructing meanings. He wants to know why a particular modernist version of the game began at a certain time, why its rules changed when they did, why certain players played it. Answers for these questions entail some concept of the relationship between experience and meaning. Jelavich attempts to address this problem at a

[17] Jelavich, *Munich and Theatrical Modernism*, 10.

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number of levels, in terms of the external impact of political events and social changes and in terms of the internal pressures of social interests, political ambitions, and psychological needs, but the conceptualization and practice of this dimension of his contextual analysis is eclectic, somewhat vague, and occasionally reductive. What is missing is a clear sense of the complexity of mediated connections between experience and meaning that would parallel the new sophistication of the analysis of meaning-construction and recognize the relative autonomy of the cultural sphere. In the case of the modernist avant-garde, it would seem especially important to reconstruct the experiential context of its players in the Bohemian worlds of disinherited intellectuals and the commercial cultural relations that transformed their created meanings into marketable commodities. What kind of experience did aesthetic modernism make meaningful? This is obviously the critical question for historians. Jelavich knows this but is unable to formulate it in a clear fashion and thus address it effectively.

Agnew's *Worlds Apart* provides an interesting complement and parallel to Jelavich's study. Ranging over two centuries of the history of meaning in the Anglo-American world with significant forays beyond these temporal and linguistic boundaries, all within the confines of 200 pages, Agnew's investigation lacks the monographic specificity and empirical density of Jelavich's book. Although he is intellectually subtle and often imaginatively original in his reinterpretations of familiar phenomena, Agnew can also be irritatingly allusive and has a tendency to engage in theorizing digressions. His general theme concerns the relationship between the human experience of the economic realities of market exchange and commodity production on the one hand and the symbolic artificial world of theatrical forms and performances on the other. Agnew claims that, in premodern cultures like medieval England, the market and the theater were segregated as "worlds apart," through elaborate rules and rituals, from the stable identities and meanings of ordinary social life. Both were places in which liminal experiences of fluid exchange, threshold crossing, and transition were confined and controlled. During the sixteenth century, however, the market gradually ceased to be a place and became a "placeless process" pervading general social relations in the culture that had previously confined it. This new pattern of social experience had its critical and threatening aspects: the dissolution of traditional notions of self-identity in the experience of a protean, estateless, masterless self always on the threshold of becoming something or someone else in the process of exchange, the breakdown of transparent reciprocity between social actors through the calculated misrepresentation of private intentions in the constantly renegotiated relations of social life, and the leveling of traditionally constituted social and cultural hierarchies through the commutation of all specific obligations and values into the liquid equivalencies of commodity exchange.

Agnew contends that English Renaissance theater, in its forms as much as or more than in the specific messages of its contents, modeled, "materialized," and explored the ambivalences of these new market relationships, giving new meaning to the ancient figure of *theatrum mundi*: "Separated like the market from its original ritual and hierarchical aegis, the Elizabethan and Jacobean theater furnished a

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laboratory of representational possibilities for a society perplexed by the cultural consequences of its own liquidity."[18] The complex analyses and interpretations with which Agnew pursues this theme through the seventeenth and eighteenth centuries cannot easily be summarized, but the guiding thread of his argument is that the cultural history of the theater is marked by persistent attempts to repress, deny, domesticate, and control the protean implications of market relations as represented on the stage. His analysis culminates in an interpretation of eighteenth-century social philosophy as a form of this domestication, in which elements of the theatrical metaphor were absorbed into social analysis for the purposes of segregating the market as an objective, economic process from the sphere of moral and aesthetic values, thus controlling the threatening experiences of a generalized market "culture."

In many ways, Agnew's study can be read as an exciting example of the practice of the new, semiologically oriented history of meanings. Throughout his argument, Agnew generally follows a deconstructive model of reading texts as complex constructions of signs attempting to impose a fixed structure of meaning on the fluidity of market experiences, constantly undermined in their attempts by the subversive power of market culture itself. He also eschews the conventional distinctions between sociocultural and intellectual history and applies the same interpretive methods to documents of popular culture as to the sophisticated artifacts of high culture. Agnew is clearly more subtle and convincing than Jelavich in his efforts to interpret the symbolic world of cultural artifacts as struggles to make experience meaningful, but he is less aware than Jelavich of the heterogeneity and peculiarly compound nature of particular meaning-constructions. Agnew reads all of his texts, whether sermons, political treatises, or dramas, as addressed to the same experiential dilemmas, with little concern for the varieties of inherited patterns of meanings or the varieties of experience that constitute the contexts of these texts. Agnew simply assumes that the primary reality underlying social experience in modern society is the progressive transformation of social relations through commodity production and exchange, producing a pervasive, problematic relationship between fluid and stable models of meaning-construction.[19] In the parlance of the new intellectual history, one could say that Agnew is not attentive enough to the specificity and plurality of historical "discourses," the heterogeneity of the questions these discourses address, and the variety of the worlds of meaningful experience that are produced, reproduced, and transformed within them.

ASIDE FROM THE REFORMULATION OF THE TEXT-CONTEXT PROBLEM, the most obvious consequence of the linguistic turn in intellectual history has been a focus on "discourse" as an organizing term for conceptualizing and practicing the history of meaning. In a concluding comment on the essays in *Modern European Intellectual History: Reappraisals and New Perspectives*, edited by LaCapra and Steven Kaplan,

[18] Agnew, *Worlds Apart*, 54.

[19] This position appears to be based on a rather simplistic, "documentary" (in LaCapra's sense) reading of Marx; compare Agnew, *Worlds Apart*, 52.

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Hayden White notes that "discourse" has become one of the most commonly used terms in the lexicon of the new generation of intellectual historians. [20] However, the essays on which this comment is based reveal at least two significantly different concepts of what a discourse is and what its acceptance as an organizing principle might imply. Mark Poster, presenting and advocating a Foucauldian perspective, insists that redefining the object of intellectual history as discourse(s) implies a radical, revolutionary break with the rationalist, subjectivist, evolutionary assumptions of the Western cultural tradition and thus also with the practices of the conventional intellectual history that have served as its preserver and mouthpiece. Discourses are identified as archaeologically recoverable, objectively describable "systems of statements" related according to rules and procedures that rigorously determine what can be said and how it can be said. They are static structures in the sense that changes can only occur as internal transformations within the rules that define them, and as discontinuous and incommensurable in the sense that they constitute self-defining worlds whose relationship to other worlds can only be construed in terms of exclusion, resistance, or domination. Individual performances or actions within a discourse are thus always instances or manifestations of its ordering regularities. At the same time, discourses are perceived as intimately tied to institutions and social practices, as forming structures of domination or systems of power. Discourses might thus be described as impersonal, anonymous, "objective" systems of rules that, in a very practical and active sense, construct the world of objects and subjects, the world of "experience."

Foucault's concept of discourse evolved in the study of disciplinary languages closely connected to the social practices of medicine, psychiatry, criminology, and sexology, and it has been most influential in stimulating historical scholarship in these areas. Poster advocates that Foucault's discourses should become the proper object of intellectual history in a broader sense, organizing the study of all patterns of meaning-construction. Such advocacy seems caught in insurmountable contradictions. Poster admits that, in the process of translating Foucault's position into terms that would allow it to function as an interlocutor in current debates among intellectual historians, he "examined the discipline of archeology not as a discourse, but as a set of ideas, as the project of an author, as the work of a subject."[21] To engage seriously in a discourse with Foucault therefore seems to assume a concept of discourse that undermines Poster's own, that recognizes the novelty and autonomy of individual discursive performances and the possibility of discursive transformation through intersubjective communication.

Although Keith Baker's contribution to the volume edited by Kaplan and LaCapra pays lip service to the theories of Foucault, the concept of discourse Baker develops for the investigation of competing ethical and political ideologies articulated in the general space of "public" culture diverges sharply from Foucault's model. Although Baker does see discourses as separately constituted

[20] Hayden White, "Method and Ideology in Intellectual History: The Case of Henry Adams," in LaCapra and Kaplan, *Modern European Intellectual History*, 280.

[21] Mark Poster, "The Future According to Foucault: The Archaeology of Knowledge and Intellectual History," in LaCapra and Kaplan, *Modern European Intellectual History*, 152.

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domains of meaning and social action in which the public world, its political hierarchies, legal structures, social categories, and relevant actors are constructed according to varying internal "logics" or sets of rules, organizing concepts, and metaphors, these discourses are not "insulated from another in any strict way." They are dynamic rather than static and undergo constant change and elaboration through the activity of the individual agents who perform within them. They are open to the impact of new experiences and are capable of responding to the actions occurring in other discourses.[22] This concept of heterogeneous, compound, interacting, open discourses in a constant state of dynamic change both within themselves and in their relations to each other because of the transformative activities of individual agents who articulate them seems to owe much less to Foucault than to the theory of discourse developed over the past twenty-five years in the historiography of early modern Anglo-American political theory, especially by Quentin Skinner, John Dunn, and J. G. A. Pocock, a theory that has recently been articulated in its most developed form in Pocock's *Virtue, Commerce and History*.

As Pocock's introductory essay, "The State of the Art," makes obvious, the theory of the history of political discourse has become increasingly complex and flexible as its practice has evolved and the number of its practitioners (now virtually a discursive community themselves) has expanded over the years. As the object of historical inquiry, discourse is differentiated into a relationship between three factors or dimensions. First, there is the "structural" dimension (developed in an extreme, one-sided fashion by Foucault) of relatively stable conventions, usages, idioms, rhetorics, or vocabularies that Pocock now refers to as "languages." (This designation is a bit confusing since "languages" do not overlap with the ordinary meaning of vernacular languages but are really sublanguages: conceptual and metaphorical frameworks that can be translated from one vernacular to another.) Languages embody the rules that define a communicative world, determining what counts as reality and limiting the possible ways in which realities can be connected. Many such languages may and usually do coexist (the "republican" language of civic virtue, the classical liberal jurisprudential language of rights and contracts, the utilitarian liberal language of interest and strategic adjustments) in the public space of political struggle and discussion at any particular historical moment, and any given text may participate in a number of languages and relate them to each other implicitly or explicitly in a variety of ways, occasionally developing "meta-languages" or secondary languages in order to do so. The first task of the historian of discourse is to identify and reconstruct such languages, to demonstrate their "paradigmatic" force or organizing power in various texts and to construct their implicit forms into an explicit "ideal type" or hypothetical model that can then become an instrument for identifying a particular language in other texts and contexts.

The second dimension in discourse is comprised of the specific linguistic performances or "speech-acts" that are not just events in language but actions on language, expropriating the inherited, already constituted framework in order to

[22] Baker, "Ideological Origins," 200–03.

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modify or transform it. Such acts are acts of communication demanding and eliciting responses or "countermoves," which are themselves creative expropriations and transformations of language. It is this process of constant interaction between speech and language, action and structure, that constitutes a "discourse." The conditions that make a discourse possible are a plurality of languages and the existence of speakers who have access to these languages and are thus able to engage in creative linguistic performances. Any single text may perform actions in many languages at once, relate different languages to each other in a single action, or even move toward the creation of new linguistic forms through the creative play of the inventory of meanings at its disposal.

Third, Pocock claims that the historicity of language in discourse cannot be understood without some concept of the relationship between language and "experience." The past, as the inherited inventory of constituted patterns of meaning, weighs on the present of the linguistic actor, precluding any direct causal determination of speech by experience or reflection of experience in speech. Yet the innovations and transformations that individual speech acts perform on inherited languages must ultimately be situated in a history of experience and related to it in a "diachronous, ambivalent and problematic" manner.[23] In Pocock's historiographical practice, economic processes and political events play an important role but largely as experiences that demand response rather than determine what that response will be.

Knowledge of the experience to which discourse responds and which it transforms into meaningful experience is itself only accessible through the mediation of texts: experience is not simply given but already worked over and mediated by language and thus as much an object of interpretation as the texts in the history of discourse. Pocock's implicit but quite obvious polemical intention to recover the republican discourse of civic humanism thus also entails a revision of previous conceptions of the history of experience in the eighteenth century. The central significance Pocock attributes to the creation of the institutions and practices of public credit in the commercial revolution at the beginning of that century justifies and reinforces the continuing relevance and importance of the language of civic virtue. His attempts to displace the individualistic natural rights and utilitarian theories of Locke and Bentham to the periphery of political discourse is tied to his attempt to displace the kinds of experiences their theories made meaningful, the experiences of commodity exchange and production, to the periphery of the history of experience as well.

Experience also enters into Pocock's history of discourse at another, more general, level. He admits that the existence of discourse itself as a historical object presupposes a specific kind of experiential "base," that is, the existence of relatively autonomous agents, sharing and able to use a variety of linguistic resources available in the public domain, and thus capable of issuing utterances and responses that are non-determined and mutually intelligible. Discourse implies the communicative context of an intersubjective community of free individuals. And

[23] Pocock, *Virtue, Commerce and History*, 29.

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if the meaning-constructions of historians are to be seen as taking the form of linguistic performances in a discourse, as Pocock seems to assume they do, they presuppose an intersubjective community of scholars. Pocock's theories often seem like theoretical reflections of familiar practices because the world they assume is also the world in which many contemporary Anglo-American historians live or think they live.

The problem raised by Pocock's work in relation to the general pursuits of intellectual historians concerns the extent to which its assumptions and methods are applicable to different kinds of inquiry situated in different communicative contexts. David Hollinger addresses this question, at least implicitly, in a number of essays. Hollinger claims that the history of discourse is in fact not so much a program for intellectual historians to follow as a description of what many of them were practicing long before the linguistic turn brought a new rigor of conceptualization to their pursuits.[24] But the diversity of these pursuits implies that "discourse" as an object of study might be extended beyond Pocock's somewhat restricted model of political discourse within a community of relatively equal and autonomous actors sharing a common inventory of languages. How broadly could one define the concept of a communicative context? Could one construct a discourse on the basis of the more general notion of common questions addressed by linguistic performers in different cultures and different times? Discourse appears to assume a community of discursive actors. How far could the boundaries of such a community be extended? Two recent cross-cultural studies of the history of specific discourses among modern European intellectuals may provide insight into the practical relevance of such questions.

IT MAY BE MISLEADING TO CONSTRUE Allan Megill's *Prophets of Extremity: Nietzsche, Heidegger, Foucault, Derrida* as an exemplar of intellectual history as the history of discourse. Megill rarely uses the term "discourse" to designate the object of his analysis and tends toward the more conventional terminology of "perspectives." He affirms his protagonists' rejection of the assumption of evolutionary, continuous history, or "historicism," and claims to be "committed" to "the structuralist and poststructuralist notion of discontinuity in history."[25] His interpretations of the texts of Nietzsche, Heidegger, Foucault, and Derrida are not conceived as reconstructions of utterances and responses, or moves and countermoves within a continuing argument or conversation, but as independent redescriptions of different perspectives that can be read and understood in isolation from one another. The "strange affinities" and "telling differences" that Megill discerns within these texts are not presented as differentiations of a common vocabulary or language. He does not engage in the reconstruction of the enabling and restrictive rules of a linguistic code for purposes of contextual interpretation. In fact, Megill generally slights and often consciously dismisses contextual analysis, his aim being not to explain certain perspectives as historical actions within specific

[24] Hollinger, *In the American Province*, 131–34.
[25] Megill, *Prophets of Extremity*, 205.

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frameworks of both discourse and experience but simply to "display" these perspectives, "lay bare" their assumptions, "point out" their affinities with other "more familiar thought," account for their rhetorical force, and "to criticize in the conventional sense."[26]

Despite such disclaimers, Megill's study does conform to the model of a history of discourse in a number of ways. He admits that the temporal organization of his reconstructions conforms to a historicist model in which Nietzsche is seen as positing a perspective that is adopted in more unequivocal fashion by Heidegger, critically developed by Foucault, and finally deconstructed by Derrida. In a number of passages, this development is even described as a four-stage "dialectic." As an identifiable developing perspective, moreover, the thought of the "prophets of extremity" emerges from a shared "problematic" and manifests common patterns that could be construed as constituting something like Pocock's "language." The problematic is the belief in the existence of a crisis in Western culture characterized by the collapse of meaningful experience, by the perception that natural and sociohistorical existence is, or has become, "derelict" or meaningless, and the conviction that all attempts to claim otherwise are illusory, self-deceiving mystifications. The response to this crisis takes the form of the development of a "language" of "aestheticism," whose elements are expropriated from their original contexts in Romantic and Idealist thought and recombined in a theory of meaning which claims that art, discourse, language, and myth make and remake the world they claim to express or represent.

Megill does attempt to interpret the evolving extremism of the concept of crisis and its aestheticist corollary within a contextual framework. This framework is defined first of all as "crisis-oriented modernism and post-modernism."[27] The four figures of the study are seen as setting the agenda as well as articulating the assumptions and working out the possibilities of a style of thinking or form of discourse Megill believes defines a whole epoch of Western history, from the late nineteenth century to the present. He claims that this style of thought is intimately connected to assumptions about meaning and meaningful experience that extend far beyond the confines of small groups of avant-garde intellectuals and artists. The language of the aestheticists has become "common coin," articulating a situation that faces a collective "us" and presenting this "us" with options for response. It is this cultural context of the individual prophets of extremity that gives historical point to their claims and motivates the need to engage their discourses. Thus Megill can interpret Derrida's deconstruction of the assumptions of crisis and the autonomy of language as opening "the possibility of a beginning: one that would liberate us from the historicism and aestheticism that, in one way or another, have dominated western thought since the beginning of the nineteenth century."[28]

[26] Megill, *Prophets of Extremity*, 186.
[27] Megill, *Prophets of Extremity*, xi.
[28] Megill, *Prophets of Extremity*, 337.

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Megill makes an effort to mediate between the apparently contradictory concepts of his aims and procedures by insisting that the discursive regularities and the contexts he posits are his own "fabrications," creative constructions rather than historical reconstructions.[29] The point of these constructions is to demystify the claims of aestheticism so that we can perceive the prophets as speaking to and at us rather than for us. The fabrications of historical reconstruction are not to be perceived as purely arbitrary; they arise from a critical engagement with the texts themselves, they are acts of critical distancing through which the texts are allowed to speak as the voices of "others" to which we are asked to respond. The language of the prophets is an "edifying discourse" that constructs new possibilities of living in the world, temporarily lifting us out of the given world of the everyday, expanding our horizons. But these discourses remain places to visit, not places to live. Megill insists over and over again that the claim by aestheticists that the world is a construction of language simply cannot come to terms with our experience of the world as determined not only by meanings but by natural and social needs. There is an intransigent, given quality in experience, a "reality" encountered in the ordinary world that is obscured and ignored in the aesthetic theory of the autonomy and creative power of meaning. One would have to be a "madman" or a "fool" to take the claims of the prophets literally.[30]

This criticism of aestheticist discourse from the standpoint of experience in the "real" world points to the major problem in Megill's work. Although experience is a central category for defining his own critical standpoint vis-à-vis aestheticist discourse, experience does not enter into his historical analysis of that discourse. Even the notion of crisis is interpreted as a fabrication or construction justifying a certain mode of discourse; it is not related in any way to a history of experience to which discourse must respond. Megill makes no attempt to situate the history of the aestheticist perspective within the history of experience, even though he insists that the aestheticist perspective implies a denial of experience. Experience enters his account only from the outside, in a completely unhistoricized fashion. The consequences of this inability to conceptualize the role of experience within the history of meaning is evident in his interpretations of the texts of the aestheticists as well. The aestheticist perspective, he concedes, has been extracted from texts in which it is often in a problematic relation to other forms of discourse. He has chosen to ignore these other languages to "display" the structure of the aestheticist perspective. But it is precisely those other languages in Nietzsche, Heidegger, and Foucault that address the issue of the relationship between language and experience or language and "reality." Thus Megill has dismissed or erased experience from both his analysis of texts and his analysis of their contexts. The result is an ahistorical "history" of meaning set in radical opposition to a dehistoricized experience.

Unlike *Prophets of Extremity*, Martin Jay's *Marxism and Totality: The Adventures of a Concept from Lukács to Habermas* was self-consciously conceived as a history of a

[29] Megill, *Prophets of Extremity*, 277.
[30] Megill, *Prophets of Extremity*, 102, 343.

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discourse, the "Western Marxist discourse of totality," even though it does not consciously follow the models of either Foucault or Pocock. Jay's use of discourse as an organizing concept is complex and multilayered, as he is concerned with the ways in which one discourse (totality) functioned within another (Marxism), but is sometimes confusing, since the boundaries of both discourses are exceptionally vague and porous. The discourse of totality can be traced back as far as classical Greece and is virtually synonymous with discussion of the metaphysical or theological foundations of Western culture, but Jay does not attempt to reconstruct it in a systematic or continuous fashion. Instead, he focuses on a number of textual events within the discourse of totality that Western Marxists had already selected as relevant to their own project or that had exerted an obvious impact on their intellectual formations. His aim is to show that, by the time of the emergence of Western Marxism after World War I, the concept of totality was already imbedded in a complex, inwardly differentiated discourse, providing various possibilities for creative appropriation. What we might call the "compound heterogeneity" of the discourse of totality is described in terms of three pairs of contrasting concepts: "latitudinal/longitudinal" (more commonly designated in the current literature as synchronic/diachronic but most simply stated as spatial/temporal), "descriptive/normative," and "expressive/decentered" (replacing the more traditional subjective/objective).

The central role that the discourse of totality plays within Western Marxism distinguishes it from previous and competing Marxisms, Jay contends, and the recognition of this centrality allows the intellectual territory of Western Marxism to be charted in new ways. First, it permits Jay to connect some of the familiar characteristics of individual thinkers conventionally classified as Western Marxists: their sociocultural status as marginal intellectuals educated in elitist academic traditions, isolated from a mass audience yet committed to revolutionary change, their interest in methodological and philosophical issues and in the cultural dimension of historical analysis, their hermetic style of writing, their openness to non-Marxist intellectual traditions concerned with the issue of totality, and thus their tendency to proliferate adjectival or hyphenated Marxisms. Second, Jay's method allows him to include within the intellectual terrain of Western Marxism individuals previously excluded because of their hostility to one particular form of totality (subjectivist or neo-Hegelian) or because of their negative assessment of the possibility or desirability of any form of historical totalization. If the tradition is conceived of as a discourse emerging from a shared focus on a problem rather than as a grouping of texts with similar solutions, "positivist" Marxists like Louis Althusser or Giovanni della Volpe or negative Marxists like Theodor Adorno can also be included in a meaningful way. Finally, Jay's focus on the discourse of totality as an organizing framework makes possible a reconstruction of the history of Western Marxism as a meaningful temporal narrative.

Jay's narrative begins with the paradigmatic articulation of a Marxism reconceived around the discourse of totality in Georg Lukács's *History and Class Consciousness*. Lukács's position is conceived as paradigmatic not only because it presented a compelling synthesis of Marxist and totalist discourse that later

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thinkers could assimilate but because this synthesis contained critical unresolved problems—concerning the status of humanity's relationship to nature within the sociohistorical process and the identity or even existence of a collective human subject through which totality in a normative sense was to be historically constituted—from which significant discourse could proceed. It "launched a problematic."[31] The narrative unfolds as a history of argumentation in which the originating paradigm is critically assimilated, reproduced, questioned, revised, elaborated, and increasingly "deconstructed" to the point where discourse of totality itself is placed in question. The story concludes, however, not with the collapse of the project of Western Marxism but with Jay's sympathetic portrayal of Habermas's "Promethean effort to reconstruct Marxist holism on boldly new grounds"[32] and a discussion of how this effort might be sustained in the face of the poststructuralist challenge to all forms of discourse of totality.

Jay's story, like Megill's, is clearly structured by its conclusion. Habermas and Derrida serve as guides to the histories that lead up to them, but Habermas's guidance is rather different than Derrida's. First, Jay is much more sensitive than Megill to the complex, conflicting relations between different "languages" or paradigms in any particular textual performance and to the ways in which the history of discourse proceeds not as a series of edifying fictions creating their own realities but as an intersubjective dialogue in which a common, shared reality is tentatively created through agreement and consensus. Second, unlike Megill, Jay places the relationship between the history of experience and history of meaning near the center of the historian's concerns. This focus is evident from his attentiveness to texts that address this issue as well as in the construction of his own text. In some ways, Jay's work is also characterized by a turn away from the social history of ideas, or at least by hesitancy concerning the possibility of carrying out its program. Jay, like Megill, rejects the claim that discourse could be reduced to ideology, to the representation of social, political, and individual experience, or to the direct expression of needs, desires, or subjective intentions. Linguistic paradigms and discursive "problematics" are seen as possessing a reality and a history of their own, as informing or shaping experience as much as being informed or shaped by it. Yet Jay does insist that understanding change in the history of meaning requires a contextual analysis that is more than intertextual, that connects meanings to experience, that does not lose sight of the fact that "living individuals" and not only texts are participants in the history of discourse. Such contextual analysis is present throughout *Marxism and Totality*, but it is not its central focus and is somewhat scattershot in character. The failure or reluctance to address the relationship between experience and meaning more systematically is especially striking in a work sympathetic (or at least not hostile) to the various Marxist and Freudian-Marxist positions of its historical protagonists. Marx and Freud (standing in here for more general sociohistorical and psychological interpretations of meaning) seem increasingly to have become not so much guides

[31] Jay, *Marxism and Totality*, 127.
[32] Jay, *Marxism and Totality*, 461.

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for investigating how life becomes text (as they were for an older generation of intellectual historians) but complex texts to be deciphered with the help of linguistic and philosophical guides.

WITHIN THE EXTENDED, LOOSELY ORGANIZED FAMILY of historiographical subdisciplines, intellectual history has gained occasional, grudging respect and, more frequently, notoriety for the intensity and promiscuity of its interdisciplinary liaisons. This is especially true for those intellectual historians who pursue what H. Stuart Hughes described almost thirty years ago as the *via regia* of intellectual history—reconstructions of the meanings and interrelations of "great" texts, which, apart from the particular case of the history of historiography, tend to be situated in the interpretive traditions, disciplinary vocabularies, and libraries of other academic departments.[33] When intellectual historians become particularly promiscuous in their interdisciplinary relations, they may be open to charges of unprofessional dilettantism; when they forge particularly intense relations in one direction, they may be accused of professional disloyalty, of not doing "real" history. Sensitive to new developments in the disciplines from which they borrow their objects of research, intellectual historians may often function as advocates of theoretical perspectives that appear alien to their disciplinary colleagues. Because of their uncertain professional and institutional status and their concern with theoretical texts, intellectual historians often tend to be more self-reflective and interested in theoretical discussion of the nature and methods of their inquiry than most other historians; they function as "cuckoos in the historical nest," in Leonard Krieger's apt phrase.[34]

Such familiar manifestations of the dilemmas and opportunities of the intellectual historian's professional situation as a scholar without a clearly defined *Fach* are evident in the works under review. But the familiar has taken on some unfamiliar forms in the context of the expansion of interest in the historical constitution of meaning within the humanities and social sciences. The trend has been away from psychological and sociological theories that provided models for relating experience to meaning in terms of representation, cause, or expression and toward theories that recognize language in all its density and opacity as the place where meaning is constituted and that have found their more general theoretical articulation in linguistics, philosophy, and literary criticism. But it is not only the direction of intellectual history's interdisciplinary liaisons that has changed. These relations also appear to have taken on a new form that is less parasitical and more reciprocal. The critical question is not whether theories, models, or methods developed in one discipline can be effectively applied in another but whether a general shift in perspective has occurred regarding the production, reproduction, and transmission of cultural meanings that impinges on all of the disciplines in the human studies and provides the basis for a genuine

[33] H. Stuart Hughes, *Consciousness and Society: The Reorientation of European Social Thought, 1890–1930* (New York, 1958), 10.

[34] Leonard Krieger, "The Autonomy of Intellectual History," *Journal of the History of Ideas*, 34 (1973): 499.

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interdisciplinary dialogue in which intellectual history will have a distinctive voice. The shift in the form of intellectual history's interdisciplinary relations is evident in three areas: in the kinds of interdisciplinary "borrowing" exemplified in the writings of LaCapra and Megill, Poster and Jay, in the type of "grand" theory most influential in the human studies, and in the internal transformations of neighboring disciplines such as philosophy and literary criticism.

For almost a decade, Dominick LaCapra has been urging intellectual historians to pay closer attention to developments in literary criticism and philosophy in order to "acquire the conceptual means to come to terms with problems in their own field."[35] More specifically, he has persistently claimed that intellectual historians could profit from the study of Derrida in order to learn the art of close and critical textual reading. From the Derridean perspective, historical texts appear as scenes of struggle between opposing tendencies and ultimately as acts of linguistic domination in which one tendency is hierarchically privileged as the source of unity and order. LaCapra believes that this perspective has significant implications for the practice of intellectual history. First, it reanimates the classic texts as vibrant inner contests or dialogues that are only artificially or politically resolved. Second, it places the intellectual historian in the position of a cultural critic whose interpretive readings involve an immanent critique of such artificial and political resolutions as acts of domination or self-deception. Third, the practice of deconstructive reading involves the reader in a dialogic relationship with the text. As an uncompleted contest, the text becomes a goad to thought, resisting the historian's own attempts at "closure," either through full appropriation or simple projection. Texts become present as active "others," not as objects or mirrors. Fourth, in the dialogue with the historical text, the interpreter becomes aware of interpretive activity as a creative, transformative appropriation as well as an act of reception and recovery. Intellectual history becomes an education in cultural self-awareness. Finally, LaCapra insists, as we have seen, a critical, deconstructive reading of historical texts reopens and "problematizes" the relationship of text and context.

The perspective on textual reading that LaCapra derives from Derrida is not simply a transplantation of certain methods and models from one discipline to another. He finds in Derrida a general attitude toward the production and reproduction of cultural meanings, a liberating, critical stance that demystifies and contests the "conventional" privileging of "unity and its analogues: order, purity, closure, undivided origin, coherent structure, [and] determinate meaning."[36] In this sense, LaCapra's position parallels that of Megill, for whom Derrida functions as the liberator from the literal claims of aestheticism. In both cases, the intellectual historian's professional role is merged into the interdisciplinary vocation of cultural criticism, whose aim is to prevent cultural closure and keep the process of dialogue and criticism open. But LaCapra perceives the intellectual historian's vocation of historical scholar to be a crucial element in the more general vocation

[35] LaCapra, *Rethinking Intellectual History*, 15.
[36] LaCapra, *Rethinking Intellectual History*, 68.

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as cultural critic. Although philosophers and literary critics may pay lip service to the need for contextual historical analysis, they are untrained and often inept in its practice. Derrida may have correctly "problematized" the relationship between text and context, but he has not provided guidelines for the way contexts (other than the general context of the Western metaphysical tradition) can be read so as to illuminate their significance in the production of "work-like" texts. Intellectual historians can provide historical specificity to the ways in which discursive codes and social practices enter into the play of meaning-construction. Because all meaning is "context-bound" and because this includes the meanings that critics produce in their deconstructive readings, LaCapra clearly envisions the intellectual historian's role *qua* historian to be more than ancillary in the interdisciplinary temple of cultural criticism.[37]

Although Jay and Poster acknowledge the general validity and historical importance of the deconstructive critique of previous attempts to construct a unified determinate meaning for experience, the inspiration each derives from Habermas and Foucault is tied more closely to the need to create some new form of cultural consensus and historical coherence in the wake of the critical carnage. Jay finds in Habermas at least the possibility of rebuilding hope for an intersubjectively generated, rational consensus through historical analysis of its experiential conditions. Although Jay does not apply Habermas's theories as models for his own historical analysis, he does view them as opening up a perspective on our present condition that could revitalize the historian's traditional interest in forging a bridge between memory and hope. Poster, whose *Foucault, Marxism and History: Mode of Production versus Mode of Information* significantly moderates his earlier extreme advocacy of Foucault's positions, appears to be moving toward a similar stance. Foucault's theory of discourse is described as complementary to Marxism in that it provides a comprehension of new forms of social experience within advanced industrial societies in which "knowledge is increasingly implicated in modes of domination,"[38] and thus also as an aid for mastering the future. Both Jay and Poster see the historian as an empirical researcher sensitive to the complex dialectic between meaning and experience who plays an important role in modifying and moderating the claims of contemporary theoreticians. Thus Poster claims that historical investigations of the discursive and experiential contexts of Foucault's disciplinary discourses have revealed the need to revise Foucault's claim that discourse constructs the objects and subjects it purports to represent.

A collection of introductory essays on nine influential contemporary thinkers, edited by Quentin Skinner under the pretentious and somewhat inappropriate title of *The Return of Grand Theory in the Human Sciences*, provides an interesting perspective on the kinds of interdisciplinary relations evident in the works of LaCapra, Megill, Jay, and Poster. The volume includes useful and occasionally

[37] LaCapra, *Rethinking Intellectual History*, 17, 69, 344–46; LaCapra, *History and Criticism*, 10, 42–43, 106–07.

[38] Poster, *Foucault, Marxism and History*, 91.

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thought-provoking appraisals of the theories of Derrida, Foucault, Hans-Georg Gadamer, Habermas, John Rawls, Thomas Kuhn, Louis Althusser, Claude Lévi-Strauss, and Fernand Braudel. It would be difficult to fault the inclusion of any of these figures among an elite group of influential theorists in the humanities and social sciences over the past twenty-five years, but problems clearly arise in the attempt to bring them together as participants in the common project of restoring "grand theory" in cultural studies. Skinner's attempt to accomplish this task in his introductory essay is exceptionally muddled and vague. Confusions arise primarily in two areas. First, Skinner defines grand theory in an irritatingly fuzzy manner that seems to include two distinct types of theories: ambitious, systematic, totalizing theories of human nature and culture on the one hand and theories that merge descriptive and normative claims and thus dissolve the conventional distinction between fact and value on the other. The first definition seems singularly inappropriate for many of the thinkers treated, especially for the critics of all totalizing theoretical claims such as Foucault, Derrida, Gadamer, Kuhn, and even Habermas. The second definition comes closer to designating a common element of the individual theories but, at least in its iconoclastic forms, does not seem particularly "grand." The second area of confusion stems from Skinner's developmental vision of grand theory's "return." He posits an original positivist and empiricist standpoint, grounded on a strict analytical separation of descriptive, empirically falsifiable, and thus "objective" theory from subjective, ethical, and metaphysical value judgments, dominant among Anglo-American social scientists during the 1950s and undermined after 1960 "by successive waves of hermeneuticists, structuralists, post-structuralists, post-empiricists, deconstructionists and other invading hordes."[39] Obscured in this metaphor is the fact that the original context of the invading theories was different from the context of the invaded theorists. It is difficult to see why Gadamer or Derrida should somehow be seen as engaged in a grander form of theorizing than should Heidegger or Sartre. More puzzling still is Skinner's suggestion that the invasion of the Continental critics of the positivist-empiricist consensus cleared the ground for new forms of systematic, totalizing theory in Althusser, Lévi-Strauss, and Braudel. This evolution is a "return" in an all-too-literal chronological sense.

Despite such confusions, *The Return of Grand Theory* does provide suggestive guidelines for grasping the overall cultural point of much contemporary theorizing, especially in the work of Derrida, Gadamer, Foucault, and Habermas, and especially as it relates to the theory and practice of intellectual history. At the most obvious level, Skinner's Continental invaders and their Anglo-American sympathizers have constructed a sweeping and apparently devastating critique of the traditional foundations of knowledge claims in both the natural and the human sciences. If we take them seriously, we must recognize that we have no access, even potentially, to an unmediated world of objective things and processes that might serve as the ground and limit of our claims to knowledge of nature or to any

[39] Quentin Skinner, "Introduction: The Return of Grand Theory," in his *The Return of Grand Theory in the Human Sciences*, 6.

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transhistorical or transcendent subjectivity that might ground our interpretation of meaning. Knowledge and meaning are not discoveries but constructions. The world and the subject that confronts it are "always already" present to us as culturally constructed. This perspective (and one may certainly call it "grand" in its sweeping reformulation of the way we live our selves and our worlds) has a number of significant implications. It is radically historicist in the sense that all knowledge and meaning is perceived as time-bound and culture-bound, but it also undermines the traditional historians' quest for unity, continuity, and purpose by robbing them of any standpoint from which a relationship between past, present, and future could be objectively reconstructed. By conceiving of knowledge as a form of action, as creation, domination, or communicative engagement, moreover, this perspective implicates all forms of knowing in the social and political practices of a specific sociocultural formation. Finally, the new grand theory tends to dissolve the analytical distinctions between, and hierarchical ordering of, different modes of knowing and the disciplines connected to them.

Within the general situation articulated in grand theory, various responses are possible. One may, like Gadamer, focus on the ways in which shared cultural meanings embodied in different traditions can be revitalized (without falling into the metaphysics of subjective identification) through the existential encounter of different cultural worlds or horizons and the production of a dialogic relationship Gadamer describes as the fusion (never complete) of such horizons. One may, like Foucault, emphasize the hegemonic aspects of all collective, organized worlds of meaning and engage in a relentless, though seemingly Sisyphean, critique of domination. One may, like Habermas, combine a critique of hegemonic discourses and their sustaining practices with a utopian projection of a possible sphere of free communicative interaction and democratic world construction. Or finally, one may, like Derrida, revel in what Nietzsche called the *fröhliche Wissenschaft* of deconstructive criticism, unveiling the proliferation of meanings without end in the repetitive, constantly displaced struggle to impose univocal meaning or cultural closure. None of these responses can be theoretically or empirically grounded, of course, without falling into the illusions of a bankrupt metaphysics; they can only be justified in practice.

Although thinkers like Gadamer, Derrida, Foucault, and Habermas are not scholars in specific disciplines, they might be described in terms of a peculiar combination of intellectual historian and cultural critic. As scholars, they engage in the historical analysis of texts, discourses, and other forms of the production and reproduction of meaning. As intellectuals, their work is aimed at the practical transformation of the present and finds its justification in that effect. One could say that they provide a self-reflective description of the practice of intellectual history as a way of life rather than a specialized academic discipline. The ambivalence produced by recognition of the general cultural perspective implied in the intellectual historian's routine working assumptions is one of the persistent themes in the essays of David Hollinger, recently collected and published under

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the title *In the American Province: Studies in the History and Historiography of Ideas*. Hollinger's field is American intellectual history, and he largely ignores the Continental invaders that dominate Skinner's conception of grand theory, but he does address the issues raised by the Skinner volume, especially in his responses to the writings of Thomas Kuhn and Richard Rorty. He perceives Kuhn and Rorty as "historian's philosophers" who articulate and defend "in a circle beyond the historical profession a basic historicism with which most historians are generally quite content."[40] But, while historians may be quite content to assume a radical historicist perspective on truth and value in their professional work, they may find themselves less content when the historicist rejection of a transcendental ground is asserted as a general philosophy and applied to their own activity as truth tellers and reconstructors of meaning, as well as to their everyday practice as cultural beings in need of cosmic supports for their ethical commitments.

In his essay on Kuhn, first published over a decade ago, Hollinger seemed to share Kuhn's assumption that "the invitation to forsake at last the fictional absolutes of natural theology" in favor of practically effective, consensually constructed standards and traditions, the "transition from transcendent objectivity to socially grounded objectivity," did not imply either for historiography or for life that "terror and caprice" would succeed rational and empirical grounding in the production and reproduction of knowledge and value. In fact, Kuhn's strong affirmation of the value and effectiveness of the scientific enterprise pursued under the aegis of consensually constructed communal standards seemed to provide a model not so much for undermining objective standards as for redescribing them in ways that could protect us from unwarranted skepticism. The historicization of natural science implied the principle that "community sanction" was "essential to knowledge" and thus pointed toward the need for disciplinary communities to set goals and standards in all fields of inquiry.[41]

In a discussion of Rorty's *Philosophy and the Mirror of Nature*, Hollinger approaches the issue of the communal validation of knowledge and value from a slightly different angle. He clearly sympathizes with Rorty's historicization of philosophy, with his critique of the "epistemological" project of ascertaining the conditions of objective knowledge, and with his proposal that philosophers should join the general interdisciplinary project of historical hermeneutics, interpreting various forms of knowledge as cultural constructions, describing the ways in which these various constructions might hang together, and thus gathering them into "the general conversation of mankind." "If Rorty's reform of his own discipline were to be actually carried out," Hollinger claims, "the voice of philosophy would begin to sound rather like the voice of intellectual history."[42] But Hollinger also expresses two reservations about Rorty's radical hermeneutics. First, Hollinger is uneasy with the demotion of science to just another voice in the cultural conversation. Hollinger does not believe in the need to preserve some place where

[40] Hollinger, *In the American Province*, 167.
[41] Hollinger, *In the American Province*, 124, 128–29.
[42] Hollinger, *In the American Province*, 169.

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universal standards still apply, but he does believe that the kind of effective knowledge promoted by the disciplinary community of natural scientists should not be slighted. Although he would rather see intellectual history defined as part of an interdisciplinary "commons"—"an intellectual expanse occupied periodically by scholars operating out of many networks and possessed of a variety of skills"—than as a professional subdiscipline or "estate," the occupants of this commons could still form a community able to give shape to inquiry on the basis of certain shared commitments. He proposes three such commitments that he believes are already implicitly shared: first, the insistence that thinking is neither a transhistorical essence nor an epiphenomenon reducible to something else but a real historical activity with events and structures organized in discourses that are related in complex ways to other forms of social action; second, the claim that the historical activity of articulating arguments in discourses by those who are producers and reproducers of meaning by vocation or profession—the intellectuals—is historically significant; and third, the operating assumption that social action takes place within enabling and limiting linguistic structures that, at the level of public discourse, implicate all inhabitants of an organized political community but are also connected in intimate ways to the discourses of intellectuals. Such commitments, he claims, are sufficient to frame the activity of a community of inquiry, delimiting problems, directing research, and establishing criteria of judgment. Only from a base in such a community of discourse and action can intellectual historians enter into the interdisciplinary and ultimately public discourse of cultural criticism with a distinctive voice.[43]

Hollinger's concern for the relationship between the discourse of intellectuals and the public discourse that informs the collective activity of politically organized communities is the basis for his second reservation about Rorty's stance—its political implications. The notion of general cultural critique is not an adequate basis for public discourse. Because community, discourse, and effective social action are intimately related, the question raised by Hollinger is actually whether consensual communities able to engage in public discourse and direct action can exist or whether consensus on the validity of knowledge and substance of value is always imposed, hegemonic, or repressive. Hollinger certainly agrees with Rorty that it is no longer historically possible or desirable to establish consensus by appealing to some metaphysical foundation; consensus must arise from within the communicative interaction of discourse itself. Rorty, much like Derrida, suggests that the only consensus possible in a post-metaphysical culture is agreement that the contest of interpretations, the play of meanings, must not be closed, that the conversation of humankind must be kept open. Hollinger, however, notes the critical importance of at least some element of substantial consensus if effective action, whether in scholarly research or in public policy, is to be possible. Both turn to the tradition of American Pragmatism to ground their perspectives. But Hollinger is more concerned than Rorty about the ways in which Pragmatist critical stances toward the epistemological project of grounding truth and value in a

[43] Hollinger, *In the American Province*, 177–81.

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transcendent sphere were tied to positive attempts to create a "culture of inquiry" modeled not on the metaphysics but on the social organization and effective practice of natural science, and in the ways in which a professionally confined discourse merged into a more general discourse among intellectuals and finally affected the form and content of public discourse.[44]

The issues at stake in the disagreements between Hollinger and Rorty should not be construed simply as differences in the professional perspectives of a historian and a philosopher. *Philosophy in History*, a volume of essays both exemplifying and advocating the "historicization" of philosophy, edited by Skinner, Rorty, and J. B. Schneewind, reveals the extent to which an expanding group of Anglo-American philosophers have become involved in similar debates. The striking thing about *Philosophy in History* is that, despite the inevitable differences in perspective one should expect in a wide-ranging book of sixteen articles by different hands, the need for a historicization of philosophy and thus for a more intimate relationship between philosophy and other social or cultural studies—and especially intellectual history—is assumed rather than debated. There is a consensus, at least among this select group of philosophers, that the discipline of philosophy as recently practiced in the Anglo-American context has been much too narrow in scope and outrageously pretentious and anachronistic in its claims. It has focused on obscure linguistic and logical issues tied to the epistemological project of articulating the conditions of certain knowledge, defined these issues as the eternal questions of philosophy, and reconstructed the history of philosophy as successive attempts to answer these questions. The historical perspective is put into operation to reveal the historically contingent nature of this definition of philosophy and philosophical problems, to open up the discipline to a range of new questions (relating to the interpretation of meaning, ethics, and politics) as well as to methods for approaching and writing about such questions. At issue is not simply the revision and enrichment of the history of philosophy through the expansion of its canon of relevant thinkers and questions, but the reformulation of the nature of philosophy as a cultural activity. The conventional analytic distinction between the validity and the historical genesis of truth claims is rejected. This does not mean, however, that all of the historicist revisionists are willing to go as far as Rorty in transforming philosophy into a mode of cultural critique. In fact, the majority of the contributors seem, like Hollinger, to assume that the collapse of belief in the metaphysical foundations of knowledge and value, and thus of the epistemological model for doing philosophy, need not imply the impossibility of reconstructing knowledge and value on historicist foundations.

The case for the moderate historicist position is presented most clearly in the lead article by Charles Taylor, which directly challenges Rorty's assumption that the collapse of the epistemological model of philosophy implies acceptance of the relativity and incommensurability of all constructions of meaning. Against such

[44] Hollinger, *In the American Province*, 3–43. See also Richard Rorty, *Consequences of Pragmatism* (Minneapolis, Minn., 1982), especially xiii–xlvii.

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"non-realism," Taylor argues that the meanings constructed by philosophers must be judged in terms of their relationship to the historical reality of social practices. The epistemological model, he contends, was an abstract, universalizing, and thus self-justifying description of a form of social action implicitly embedded in a wide range of technological, ethical, and political practices. Criticism of this model originated in the encounter with the practices it justified and the growing recognition and self-assertion of alternative forms of social practice that the epistemological model could not assimilate or make intelligible. Philosophical activity as the historical criticism of cultural assumptions brings to articulation kinds of experience that have been repressed or ignored in the conventional culture. It refutes the previous model by demonstrating its inability to provide experience with intelligibility. Skepticism and "undecidability" could only arise from a critique of a cultural framework claiming exclusivity if the critic were without communal ties, without a historically situated life in specific social practices. But criticism of the epistemological model is also a criticism of the possibility of such a historically disengaged, culturally "homeless" stance. The recognition that philosophy is "inherently historical" is also a "manifestation of a more general truth about human life and society." Taylor thus argues that it is possible to attain rational consensus or at least maintain a rational discourse about the relative validity of alternative patterns of cultural meaning. This possibility in turn is based on the intimate connection between meaning and experience, between discourse and communal practices.[45]

TAYLOR'S CRITIQUE OF RORTY BRINGS US BACK to the major issues raised by the linguistic turn in intellectual history. There can be no question that this turn has enormously enriched our historical understanding of the complex ways in which meaning is constituted, transmitted, and transformed in the heterogeneous, compound, interrelated worlds of meaning we call culture. The tides of psychological and sociological reductionism seem to have been dammed and turned back. The history of meaning has successfully asserted the reality and autonomy of its object. At the same time, however, a new form of reductionism has become evident, the reduction of experience to the meanings that shape it. Along with this possibility, a new form of intellectual hubris has emerged, the hubris of wordmakers who claim to be makers of reality. Modern European intellectual historians seem especially sensitive to these trends and the pretensions and anxieties they encourage. If the books under review are any indication, current work among intellectual historians of the younger (post-1968) generation reveals a pressing need to rethink the relationship between experience and meaning with the same critical intensity and sophistication that has been devoted to exploring the ways in which meaning is constituted in language. Such reconsideration will also entail greater attentiveness to the continuities that join the work of the younger generation, in both methods and problems, to the so-called conventional history

[45] Charles Taylor, "Philosophy and Its History," in Rorty, *Philosophy in History*, 17–30. An essay by Alasdair MacIntyre in the same volume makes a similar argument.

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of older generations. Renewed focus on the experiential component in the dynamic, mutually implicated polarities of meaningful experience is not simply a matter of reclaiming a balance lost in recent oscillations between opposing reductionisms. It is essential for our self-understanding, and thus also for fulfilling the historian's task of connecting memory with hope, that we recognize and examine the recent turn away from experience as a specific response to particular events and developments in the history of experience, a response, to be sure, burdened, limited, and shaped by the already constituted, inherited world of meanings in which, and from which, it was constructed.

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Review Article

Positivism, Relativism, and Narrative in the Logic of the Historical Sciences

RICHARD HARVEY BROWN

C. Beham McCullagh, **Justifying Historical Descriptions** (Cambridge University Press, 1984).

Ernest Gellner, **Relativism and the Social Sciences** (Cambridge University Press, 1985).

SCHOLARS HAVE LATELY GIVEN MUCH ATTENTION to the logic of method in historical studies. Epistemological relativists such as Peter Winch, Karl-Otto Apel, and Paul Feyerabend have challenged the positivist approach, and neopositivists have clarified and defended their paradigm in response.[1] Some of these positions are represented in Beham McCullagh's *Justifying Historical Descriptions* and Ernest Gellner's *Relativism and the Social Sciences*. McCullagh's book is a thoughtful exposition in support of positivist historical science, and his scrupulous argumentation also reveals the limits of his paradigm. In contrast, Gellner's work starts by accepting the limits of positivism but ends by resisting the temptations of relativism. Gellner takes the relativists seriously but uses their arguments to defend the cognitive superiority of a neopositivist critical rationality. I will review each of these works as a counterpoint to the other and as two moments in a larger dialectic between positivist materialism and romantic idealism.

Justifying Historical Descriptions is at once broad-ranging in content and limited in perspective. McCullagh proposes "to discover the logical conditions under which people are justified in believing historical descriptions to be true."[2] This pursuit is rooted in McCullagh's commitment to a positivist ontology and an empiricist epistemology. He believes that a perceptually available world exists and is structured independently of our concepts of it and that the truth of statements lies in their correspondence with this external reality.[3]

[1] Peter Winch, *The Idea of a Social Science* (London, 1958); Karl-Otto Apel, *Die Idee der Sprache in der Tradition des Humanismus von Dante*, Archiv für Bergiffsgeschichte, vol. 8 (Bonn, 1963); and Paul Feyerabend, *Against Method: Outline of an Anarchistic Theory of Knowledge* (London, 1978).

[2] C. Beham McCullagh, *Justifying Historical Descriptions* (New York, 1984), 10.

[3] McCullagh, *Justifying Historical Descriptions*, 1.

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McCullagh notes some dissent from this vision by cognitive relativists, but he does not discuss the revolution in philosophy of the past fifty years that has largely overthrown the metaphor of an absolute foundation to knowledge and replaced it with a concept of knowledge as emerging from linguistic practices. McCullagh does not discuss, for example, the idea that discourses (rather than logic and facts as such) may sustain claims to truth within their domains of application. Instead, he simply asserts without argument his traditional empiricist postulates that relativist positions are either idealist or fatally self-contradictory; that to abandon logical empiricist canons of truth is to invite their opposite—ideology and propaganda; that, by implication, since the book is about historical descriptions and not explanations, empiricism provides minimal criteria for accepting the truth of descriptions regardless of one's other epistemological views concerning historical interpretation or analysis; and that, even though historical beliefs about the past may be false in a strictly empiricist sense, "for practical reasons it is useful to accept them as true . . . The fact that such descriptions are judged to be strictly false is out-weighed by their practical utility, which justifies their acceptance as true."[4]

McCullagh presents these assumptions in the opening chapter, "Truth and Justification." He does not seriously defend any of these four claims, though all are dubious on one ground or another. Concerning idealism and contradictions allegedly inherent in cognitive relativism, for example, McCullagh could have explored with profit the nonanalytic traditions of the philosophy of knowledge, such as hermeneutics, phenomenology, pragmatism, or ordinary language philosophy, or the works of Dilthey, Heidegger, Gadamer, Ricoeur, Habermas, or Betti.[5] None are cited or discussed, but all have significant things to say about the empiricism to which McCullagh is wedded and about the argument of practical utility that he asserts in its defense. Omissions such as these reinforced my skepticism as I read further into McCullagh's treatise. Yet I found myself little by little seduced by the consistent clarity of his writing, the prudence of his arguments, his mastery of a wide variety of logical forms, his enormous erudition, and his ingenious use of examples of actual reasoning by historians.

Another merit of the volume is its well-balanced structure. It begins with an introduction (discussed above) and closes with an epilogue (Chapter 9) called "Truth and Interpretation in History." Chapters 2 through 4 treat various logical methods of justification in historical description. Chapter 5 is a bridge to what follows, for it illustrates further how these various methods are combined, used, or misused in actual practice by historians. Chapters 6 through 8 consider the logic

[4] McCullagh, *Justifying Historical Descriptions*, 2, 3, 5.

[5] Wilhelm Dilthey, *Gesammelte Schriften* (Stuttgart, 1957); Martin Heidegger, *Being and Time* (New York, 1962); Hans-Georg Gadamer, *Truth and Method* (New York, 1975); Paul Ricoeur, *History and Truth*, Charles A. Kelbley, trans. (Evanston, Ill., 1965); Paul Ricoeur, *Time and Narrative*, Kathleen McLaughlin and David Pellauer, trans. (Chicago, 1984); Jürgen Habermas, *Knowledge and Human Interests* (Boston, 1968); and Emelio Betti, *Teoria generale della interpretazione*, 2 vols., A. Giuffrè, ed. (Milan, Istituto de Teoria della Interpretazione, 1955), translated and abridged to one volume as *Allgemeine Auslegungslehre als Methodik der Geisteswissenschaften* (Tübingen, 1967). For overviews of this discourse, see Joseph Bleicher, *Contemporary Hermeneutics: Hermeneutics as Method, Philosophy, and Critique* (London, 1980); and Richard Palmer, *Hermeneutics: Interpretation Theory in Schleiermacher, Dilthey, Heidegger, and Gadamer* (Evanston, Ill., 1969).

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and arguments involved in making generalizations and justifying judgments of causality in history. They are closely reasoned and illustrated with examples from the literature. McCullagh presents a wide range of the forms of historical argumentation with great care and modesty, all within the framework of his initial empiricist assumptions.

To review in more detail, Chapter 2 reveals how "singular descriptions" may be justified with "arguments to the best explanation." Singular statements are those "which can be true of only one thing on one occasion, whereas a general statement is about a kind of thing or event which can have more than one instance."[6] Arguments for the best explanation have sometimes been called "hypothetico-deductive," but they include more than the formation of hypotheses and the deduction of falsifiable inferences. Best arguments must also have greater explanatory scope and power and be more plausible and internally consistent than alternatives.[7] The applications of these and other criteria of best arguments are elaborated and illustrated with examples from the writings of historians. Even so, McCullagh concludes that the truth of the best arguments is still "something we just accept," since, as Popper said, "We do not know: we can only guess."[8]

The third chapter presents statistical inference, a kind of reasoning by analogy from many similar cases, as another way of justifying singular descriptions. This chapter is largely indebted to Carl Hempel's account of the statistical syllogism as it is used, or should be used, by historians.[9] McCullagh offers an example: "In thousands of cases, the letters V.S.L.M. appearing at the end of a Latin inscription on a tombstone stand for *Votum Solvit Libens Merito*; From all appearances the letters V.S.L.M. are on this tombstone at the end of a Latin inscription; Therefore these letters on this tombstone stand for *Votum Solvit Libens Merito*."[10] Of course, much more could be said about historical inference, particularly when aggregate data are involved or probability sampling is necessary. McCullagh explores such issues in sections on weighting or combining different probabilities, the Bayes Theorem, and the criticisms and problems involved in drawing statistical (or intuitive) inferences.

Two other ways of justifying singular descriptions, discussed in Chapter 4, are arguments from criteria and arguments from analogy. Arguments from criteria invoke rules that state "the truth conditions of descriptions."[11] For example, one rule concerning historical statements about mortality might be that "a man has died if his obituary is reported in the newspaper." Arguments from analogy resemble both statistical syllogisms and arguments from criteria, in that "their conclusions are not always worthy of belief, but may be rationally accepted if certain [other] conditions are satisfied."[12]

[6] McCullagh, *Justifying Historical Descriptions*, 8.
[7] McCullagh, *Justifying Historical Descriptions*, 18–19.
[8] McCullagh, *Justifying Historical Descriptions*, 38, 40; Karl Popper, *The Logic of Scientific Discovery* (London, 1968), 278; and Murray G. Murphey, *Our Knowledge of the Historical Past* (Indianapolis, 1973).
[9] Carl G. Hempel, *Aspects of Scientific Explanation* (New York, 1965).
[10] McCullagh, *Justifying Historical Descriptions*, 47.
[11] McCullagh, *Justifying Historical Descriptions*, 74.
[12] McCullagh, *Justifying Historical Descriptions*, 85.

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Chapter 5, "Some Common Inferences in History," shows through detailed examples how working historians use these various arguments, and it suggests indirectly that historical description of singulars is inevitably intertwined with assumptions, interpretations, and explanations of general phenomena and principles. These points are taken up expressly in Chapter 6, on "Historical Generalizations," which the author defines as statements in which "a certain proportion of things which are members of one class (the reference class) are also members of another (the attribute class)."[13] An example of this type of generalization would be: "Most eighteenth century [English] industries depended directly on agriculture for their raw materials."[14] McCullagh notes many forms of historical generalization, such as singular hypotheticals, accidental and law-like generalizations, and generalizations seeking universal application. Historians justify such generalizations variously by an "exhaustive examination of the reference class," by statistical methods, or by reference to the laws of nature or society.[15]

In considering justifications for "singular causal judgements in history" in Chapter 7, McCullagh considers at length the concept of causality as discussed mainly by critical rationalist and neopositivist philosophers.[16] He shows how their epistemological ideals are pursued in writings by historians. Curiously absent from this chapter is a discussion of the logic of controlled comparative analysis as practiced by historical sociologists such as Max Weber and Frederick Teggert or, more recently, by Barrington Moore, Philip Abrams, and many others.[17] Instead of discussing precisely the kind of comparative historical scholarship that seeks and often discovers causal generalizations, McCullagh asks us "to defer to expert knowledge" of particular historical cases.[18] In Chapter 8, McCullagh extends his analysis to "the most significant" cause, that is, to multifactorial analyses of historical events.[19] This chapter, like the others, is a model of careful exposition of neopositivist principles of explanation as they are used in historical writing.

The contradictions between McCullagh's critical empiricist epistemology and his narrow positivist ontology are most apparent in the epilogue, "Truth and Interpretation in History." Heidegger and Gadamer, with their view that "every description of reality is an interpretation" are mentioned only to be dismissed because they do not allow for "definitive descriptions" as opposed to "mere interpretations." We see here a highly sophisticated neopositivist who, through consistency and rigor, has elaborated the logic of historical method far beyond his initial empiricist assumptions. In this elaboration, however, the difference between

[13] McCullagh, *Justifying Historical Descriptions*, 130.
[14] Phyllis Deane, *The First Industrial Revolution* (Cambridge, 1965).
[15] McCullagh, *Justifying Historical Descriptions*, 140.
[16] McCullagh, *Justifying Historical Descriptions*, 171.
[17] Max Weber, *The Methodology of the Social Sciences*, Edward A. Shils and Henry A. Finch, trans. (New York, 1949); Max Weber, *The Sociology of Religion*, Ephraim Fischoff, trans. (Boston, 1963); Max Weber, *Economy and Society*, 3 vols., Guenter Roth and Klaus Wittich, eds. (New York, 1968); Frederick John Teggert, *Theory and Processes of History* (Berkeley, Calif., 1941); Barrington Moore, *Social Origins of Dictatorship and Democracy* (Boston, 1966); Philip Abrams, *Historical Sociology* (Ithaca, N.Y., 1982); and Arthur L. Stinchcombe, *Theoretical Methods in Social History* (New York, 1978).
[18] McCullagh, *Justifying Historical Descriptions*, 192.
[19] McCullagh, *Justifying Historical Descriptions*, 194.

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fact and explanation becomes blurred, and the author's discussion of logical validity becomes a discourse on hermeneutics in all but name. Finally, McCullagh allows, "it should now be clear that historical descriptions are all interpretations."[20] Yet he has rejected or ignored the idealists, the language relativists, the phenomenologists, the dialecticians, and the hermeneuticists, thereby rendering himself incapable of accounting for these transformations.

How might the contradictions in McCullagh's discourse be more fully articulated and possibly resolved? Ernest Gellner's works provide an important resource for such a project. Gellner is one of the most intellectually delightful and instructive social thinkers writing in English today. A Central European transplanted to England, a philosopher and a field anthropologist, he uses each of his cultures and disciplines to ironically illuminate the other. He combines the wit and sophistication of Continental *belles-lettres* with the rigor and clarity of empirically oriented British thought. He does philosophical analyses of Algerian tribesmen and anthropological interpretations of Oxford analytic philosophers. His rhetorical strategy is to unfold an issue slowly, spreading out its major premises and contentions. Then he moves to a dialectically higher level of discourse and uses the best arguments of his interlocutors to unmask the limits of their own positions. The issues discussed in this fashion are not resolved so much as reframed, sometimes to show the shallowness of portentious debates, sometimes to reveal the ethical or historical significance of a technical philosophical point. Gellner is a thought doctor helping us to recognize and overcome our intellectual bad faith.

Throughout his major writings, Gellner's central theme has been the social and historical emergence of forms of collective consciousness, particularly the modern scientific consciousness in relation to traditional, mainly non-Western, social and cognitive orders. In *Thought and Change* (1964), he reflected on the nature of historical process generally and on our specific ways of understanding it, seeking, in effect, Popperian answers to Hegelian questions. In *Cause and Meaning in the Social Sciences* (1973), he "sociologized" all sorts of philosophic positions even while analyzing different sociological theories from a philosophic viewpoint. Philosophy and anthropology were even more fully joined in Gellner's meditative analysis, *The Legitimation of Belief* (1975).[21] In that work, he undercut the absolutist claims of positivist philosophy of science by showing it to be, anthropologically, a normative ethics of cognition. At the same time, however, Gellner partially validated this philosophy on pragmatic and phenomenological grounds. Such a justification is especially attractive because it horrifies both positivists *and* phenomenologists. Gellner's models of historical transformation, especially that of modernization, are elaborated concretely in *Saints of the Atlas* (1969), *Muslim Society* (1981), and *Nations and Nationalism* (1983). These themes and variations also appear in *The Devil in*

[20] McCullagh, *Justifying Historical Descriptions*, 232, 233.

[21] *Thought and Change* (Chicago, 1964); *Cause and Meaning in the Social Sciences* (London, 1973); and *Legitimation of Belief* (Cambridge, 1975).

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Modern Philosophy (1974), *Spectacles and Predicaments* (1980), and in this recent work, *Relativism and the Social Sciences*.[22]

Gellner has an epistemological commitment to empiricism, a moral commitment to modernity, and a political commitment to liberalism, and he views critical scientific thought as fundamental to all three, though rarely for conventional reasons. Thus Gellner is a rhetorically sophisticated skeptic *malgré lui*, a relativist who appreciates the findings of comparative anthropology, the sociology of knowledge, and the revolutions of idealist philosophy, yet he refuses to accept that all forms of life are equally valid, that there are no transcultural standards of judgment, that paradigms are incommensurable, or that science has no cognitive superiority over other forms of knowing.

Part of the motivation for Gellner's quest is his awareness of the traumatic impact of modernization on traditional societies and the fragility of freedom in the modern world. Paradoxically, he notes, science (as the essential form of thought for the modern age) is being challenged most in the West just as non-Western peoples are most eager to emulate it. But what precisely is science? What can we know better through science than by other means? What is the difference between scientific thought, culture, and practice and nonscientific ways of thinking, symbolizing, and acting?

Gellner maps these questions by noting two divides, one deep, between pre-science and science, the other shallow, between nature and culture. In reference to the first, Gellner shows that the relativism of Kuhnian theories of science applies only to the post-scientific side of the divide. Yes, there is relativism within science, too. This, for Gellner, unlike Kuhn, is part of its self-critical powers. But Kuhn's theories tell us nothing of the emergence of science from pre-science nor anything of the cognitive superiority of science, which is the chief reason for its great stature in the world today. The second divide, that between nature and culture, is also expressed in the distinctions between the natural and the human sciences, between positivism and romanticism, empiricism and idealism, between the modernism of St. Simon and Comte and the neoprimitivism of Herder or Rousseau. Gellner is no absolutist, but he does seek an intellectual warrant for preferring some beliefs and ways of knowing to others, and he wishes this justification to be critical and not entirely self-endorsing. This is the unifying theme of the seven essays gathered in *Relativism and the Social Sciences*.

The first essay of this volume enters the Popper-Adorno dispute, a treacherous territory filled with bogs and slime. Gellner begins by unraveling the self-contradictions of the participants and their misunderstandings of each other, and he ends by defending Popper with arguments taken from the critical theorists. In the next essay, Gellner uses the early Popper to skewer Popper the later.[23] The

[22] *Saints of the Atlas* (Chicago, 1969); *Muslim Society* (Cambridge, 1981); *Nations and Nationalism* (Ithaca, N.Y., 1983); *The Devil in Modern Philosophy* (London, 1974); and *Spectacles and Predicaments* (Cambridge, 1980).

[23] A chronological stratification of the early Popper and later Popper is found in Imré Lakatos, "Criticism and the Methodology of Scientific Research Programmes," *Proceedings of the Aristotelian Society*, 69 (1968–69); W. W. Bartley, "Theories of Demarcation between Science and Metaphysics," in *Problems in the Philosophy of Science*, Imré Lakatos and Alan Musgrave, eds. (Amsterdam, 1968); and

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third essay recasts entirely the terms of the first two by showing how problems of relativism are part of the more embracing philosophic question of universals. Although any claims to universalism can be made relative through criticism, says Gellner, relativistic discourse itself depends on or implies some universals, however difficult it may be to render these explicit. Chapter 4 brings these philosophic chickens back to their social science roosts. Even though the value of the social sciences as sciences is more a promissory note than a payoff, Gellner finds idealist skepticism to be even less promising than positivism and, moreover, logically self-defeating. Chapter 5 continues the same argument by examining French structuralism as an expression of the idealist relativism criticized in the prior chapter. Chapter 6, "No *haute cuisine* in Africa," takes apart a specific application of this structuralist approach by Jack Goody. In the same spirit, the last chapter, "Concepts and Community," critically assesses Wittgenstein's notion of "forms of life" as it is invoked by many idealists, historians, anthropologists, and sociologists. This notion, argues Gellner, is not only bad philosophy but also regressive social science. There are indeed "forms of life," he shows, but these systems of meaning used to be called "cultures" and were studied empirically. Whereas cultures are distinct and in some ways unique, they generally are not incommensurable isolates, as the Wittgensteinian idealist would have us believe.

Although these remarks may give a sense of Ernest Gellner's overall project and the spirit of *Relativism in the Social Sciences*, they hardly suggest the richness and originality of his closely reasoned essays. Gellner's writings often commence as a review of a specific work, identify a more embracing theme implied in the subtext, and then expatiate on this theme through critical analyses of explicit substance and implicit ideology. Using Gellner's own method, we could say that the subtext of *Relativism in the Social Sciences* consists of the struggle between positivist modernism and romantic antimodernism, a struggle that divides the social sciences, and Western culture more generally, and that today fragments the entire world of thought. This is Gellner's central theme from the first chapter (which ostensibly reviews *The Positivist Dispute in German Sociology*) to the last (on Kripke's *Wittgenstein on Rules and Private Language*).[24]

How can this fissure between positivism and romanticism be closed? Gellner understands that describing knowledge and experience as symbolically constructed provides a way to break the spell of the positivist epistemology that is a white man's magic, a cognitive ethic dressed up in the language of logic or natural science. But he also knows that knowledge and experience cannot be exhausted by, and are not necessarily consistent with, the version of them given by the members of any particular culture, for to believe this would invoke another form

Ernest Gellner, *Relativism and the Social Sciences* (Cambridge, 1985), especially 34–39. The exemplary work for early Popper is *The Logic of Scientific Discovery* (New York, 1959); the later Popper is represented by his contributions to *The Positivist Dispute in German Sociology*, by T. W. Adorno, Hans Albert, Ralf Dahrendorf, Jürgen Habermas, Harold Pilot, and Karl Popper (London, 1976).

[24] Adorno, *The Positivist Dispute*; and Saul A. Kripke, *Wittgenstein on Rules and Private Language* (Cambridge, Mass., 1982).

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of magic—hermeneutics as hermeticism.[25] Both these magics confer a perfect coherence on historical practice; both transform the methodological postulates of Explanatory Man or Understanding Man into epistemological conclusions and equate intelligibility with ontology. Social orders do this, but critical thinkers must avoid it. Instead of acting like shamans and priests, helping to produce a unity of mental structures and social structures, social theorists can assume a critical posture by showing how such unities, whether positivist or hermeneutic, help to make the arbitrariness of any social order appear to be natural and inevitable.

On the one side, then, are old-fashioned positivists who want not only explanatory laws to which everyone must agree but also laws that assume a purely material world and deny all subjective reality, even that of the theorist. Opposing them are romantic idealists who want no laws but who prefer instead experience and intuitions, which, they claim, are a higher kind of science. Transcending both these views would be a theoretical practice that seeks generalized explanations of the natural as well as of the cultural world, including subjectivity but without a reductive materialistic or mechanistic bias.

Ironically, the program of earlier positivists to reduce our vague language to instant-by-instant experience is precisely the problem of setting the abstractions of society and history on an experiential basis. Only now this empiricism need not be either objective or subjective but can be intersubjective and social. Informed by Continental and pragmatic thought, contemporary empirical research may abandon the idea of a self-subsisting social world-out-there, a world whose data spoke for themselves before any theorizing took place. Instead, we can recognize that scientific reality is itself symbolic, that validity always depends on the coherence of our ideas and perceptions, and that the power of a theory is its capacity to produce coherence most strikingly and economically. This being the case, the methods one uses to gain agreement about a phenomenon depend not on absolutist definitions of knowledge but on the kind of explanation one chooses to seek.

IF WE ACCEPT THIS FORMULATION, derived in large part from Gellner's thought, we could use it to broaden McCullagh's perspective and eliminate its basic inconsistency, even while preserving its rigor within the positivist paradigm. First, we would enlarge the scope of McCullagh's discourse and method so that it includes approaches to historiography other than the empiricist one. Second, we would seek criteria of adequacy within historical narrative itself, rather than envisioning historical writing as a pale shadow of scientific truth.

One way to enlarge McCullagh's discourse is to understand it as a justification for only one possible way of historical figuration—the kind that recounts history on the model of the machine. Yet there is more to history than this type of account,

[25] A. J. Festugière, *La Revelation d'Hermes Trismegiste*, 4 vols. (Paris, 1950–54); and Frances A. Yates, *Giordano Bruno and the Hermetic Tradition* (Chicago, 1964).

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other ways to constitute a field for historical practice besides neopositivism.[26] For example, structuralist social historians, such as those of the *Annales* school, constitute their subject matter as a system of synchronous homologies and transformations, much as do structural anthropologists such as Claude Levi-Strauss or Pierre Bourdieu. Even though McCullagh mentions the topics of these historians with respect, he does not concern himself with their logic of method. To enlarge McCullagh's perspective, then, we would have to recognize that historiography—whether neopositivist, structuralist, phenomenological, or whatever—is unavoidably conducted through language and that, within language, a number of valid alternative methods of historical reconstruction are available. Positivist empiricism is only one.

Given such a perspective, it becomes possible to look for epistemological justifications within the method of storytelling itself. Indeed, we might precisely reverse McCullagh's assumption. Instead of reducing historical narrative to neopositivist logic, we could subsume positive science itself under the logic of narrative. This position seems bizarre at the outset, but I believe I can defend it by arguing for three subsidiary claims: that narrative logic is universal, and, hence, other logics derive from it; that epistemological crises in the philosophy of positive science are conflicts of narrative traditions; and that paradigm shifts in science itself are reformulations of cognitive traditions in terms of narrative logic.

Unlike formal rationality, which requires special training and is limited to a specific culture, class, or gender, the logic of narrative appears to be a human universal. Because human beings are linguistically constituted animals, we come to know narrative discourse through normal socialization. This concept of verbal socialization is expressed in Kenneth Burke's notion of life as an "unending conversation," in Michael Oakeshott's idea of culture as "the conversation of humanity," in Martin Heidegger's view that "we are a conversation . . . Conversation and its unity support our existence," and in Walter Fisher's arguments that "symbols are created and communicated ultimately as stories meant to give order to human experience . . . in communities in which there is sanction for the story that constitutes one's life."[27]

In the same spirit, scholars of comparative archaeology, literature, and anthropology report that the story exhibits narrative probability and fidelity across time and culture. Hayden White: "Far from being one code among many that a culture may utilize for endowing experience with meaning, narrative is a metacode, a human universal on the basis of which trans-cultural messages about the shared reality can be transmitted . . . The absence of narrative capacity or a refusal of narrative indicates an absence or refusal of meaning itself." To Dell Hymes, "the narrative use of language is not a property of subordinate cultures, whether folk,

[26] Richard Harvey Brown, *Society as Text: Essays on Reason, Rhetoric and Reality* (Chicago, 1987).

[27] Kenneth Burke, *The Philosophy of Literary Form* (New York, 1957), 94–97; Michael Oakeshott, "The Voice of Poetry in the Conversation of Mankind," in Oakeshott, *Rationalism in Politics and Other Essays* (London, 1962), 199; Martin Heidegger, *On the Way to Language* (New York, 1982), 278; and Walter R. Fisher, "Narration as a Human Communication Paradigm: The Case of Public Moral Argument," *Communication Monographs*, 51 (1984): 7.

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or working class, or the like, but a universal function."[28] Narrative enables us to understand the actions of others and endow them with meaning because it is through narratives that we ourselves live and understand our own existence.

If narrative is a universal logic of human life, what is its role in crises of epistemological faith? Can we show that epistemological crises in philosophy and paradigm shifts in science can be subsumed under narrative logic? To answer these questions, following Alasdair MacIntyre,[29] we might examine the radical doubt of two exemplary figures in the philosophy of science, Descartes and Hume. Descartes thought he had resolved his epistemological crisis purely in reason. In fact, his "resolution" is founded in logical error, religious faith, and historical amnesia. The *ergo sum* of Descartes' foundational axiom—I doubt (think, reason, etc.), therefore I am—does not follow from its premise. All that can be logically concluded from the existence of doubting is that there is doubting, not that there is a doubting being, an agency of this doubt. Furthermore, having established the *cogito*, Descartes has no way of assuring himself in reason that cogitation and reality have anything to do with each other. He relies on his faith that a benevolent God must link reason with the world.

Descartes did not really doubt everything. Self and God remain necessary, albeit undemonstrated, categories of his system. After all, how could these categories be validated by formal reason alone? Descartes was susceptible to facile resolutions because he depended on but did not acknowledge the very intellectual tradition within which he was working. He was able to resolve his epistemological doubt because he did not engage in historical doubt. He did not even put into question that he was cogitating within a tradition of Latinate realism, that what he took to be spontaneous representations of his own mind were reproductions of sentences and phrases from his forgotten-but-still-imprinted childhood schoolbooks, that he had inherited a certain epistemological ideal, and that, for him, knowledge was akin to vision.[30]

In facing a similar epistemological crisis, Hume described his own philosophic despair: "The understanding, when it acts alone, and according to its most general principles, entirely subverts itself, and leaves not the lowest degree of evidence in any proposition, either in philosophy, or common life . . . The *intense* view of these manifold contradictions and imperfections in human reason has so wrought upon me, and heated my brain, that I am ready to reject all belief and reasoning, and can look upon no opinion even as more probable or likely than another. Where am I, or what? From what causes do I derive my existence, and to what condition shall I return? Whose favour shall I court, and whose anger must I dread? What beings surround me? and on whom have I any influence? I am confronted with

[28] T. Jacobsen, *The Treasures of Darkness: A History of Mesopotamian Religion* (New Haven, Conn., 1976); Hayden White, "The Value of Narrativity in the Representation of Reality," *Critical Inquiry*, 7 (1980): 6; and Dell Hymes, "A Narrative View of the World," in Hymes, *Language in Education: Ethnolinguistic Essays* (Washington, D.C., 1980), 132.

[29] Alasdair MacIntyre, "Epistemological Crises, Dramatic Narrative, and the Philosophy of Science," in *Paradigms and Revolutions: Applications of Thomas Kuhn's Philosophy of Science*, Gary Gutting, ed. (Notre Dame, Ind., 1980).

[30] MacIntyre, "Epistemological Crises," 60.

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all these questions, and begin to fancy myself in the most deplorable condition imaginable, inviron'd with the deepest darkness and utterly depriv'd of the use of every member and faculty."[31] Hume's epistemological crisis was not resolved in the terms that he had hoped, for he (like McCullagh) established criteria of reason and knowledge that no system of belief, "in philosophy or common life," could possibly meet.[32] Hume did not ask what form of reason would be adequate to the questions he posed in terms of the specific social or historical context in which he found himself. Instead, he wanted answers that would be always and universally true. Such knowledge is available only to God. By contrast, the knowledge that humans may possess is mediated by language, construed through some point of view, and hence always limited and partial. Hume invited despair because he failed to provide himself with a narrative account of his own historical situation, an account that would have recognized the projective dimensions of language and provided him with usable canons of adequacy for his rational discourse.

Let me summarize the purpose of these two cases for my own argument. The frailty of Descartes' and Hume's resolutions of doubt is due to their failure to formulate their problematics in terms of a narrative account of their own historical situations. Had they done so, they would have seen that their epistemological crises were crises in specific intellectual traditions and that any resolution required a reformulation of the tradition. In other words, the crises and their resolutions would have been couched in narrative terms. Epistemological crises are experienced by individuals but take place within historical cognitive traditions. The mode of articulation and awareness of these traditions is narrative logic. Resolutions of the epistemological crises of individuals therefore will be either specious, as with Descartes, or impossible, as with Hume, as long as they are not cast as part of a narrative of the relevant tradition and its adequate reformulation.

Much the same is true of paradigmatical crises. Just as Descartes and Hume recounted their own epistemological crises as though they had occurred outside any tradition, so contemporary philosophers of science such as Feyerabend, Hanson, Kuhn, and Polanyi discuss crises of paradigms as moments of abrupt discontinuity with tradition.[33] In doing this, however, such writers fail to account for the continuity of intellectual traditions that is the precondition for the intelligibility of their own historical narratives of changes in paradigms. "Tradition," of course, is not a monolith that presses against the present. Instead, as anthropologists and historians have shown us, it is as diverse as the present itself, because every group in the present may seek to legitimate its contemporary claims by advancing its own version of the past. Because scientific paradigms constitute and are constituted in traditions, struggles between paradigms become debates over which tradition is more legitimate or adequate. Such discourse of traditions is a form of historical reason, a discourse that takes a narrative form. In times of

[31] David Hume, *Treatise on Human Nature*, L. A. Selby-Bigge, ed. (London, 1941), 267–69.
[32] MacIntyre, "Epistemological Crises," 58–61.
[33] Feyerabend, *Against Method*; Norwood R. Hanson, *Patterns of Discovery: An Inquiry into the Conceptual Foundations of Science* (Cambridge, 1958); Michael Polanyi, *Personal Knowledge: Toward a Post-Critical Philosophy* (Chicago, 1958); and Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago, 1972).

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dominance of a particular scientific paradigm, narrative discourse may be dismissed by those adhering to a positivist mode of logic. But, when paradigms clash, the intelligibility of each to the other and the victory of the emergent one depend on narrative form. The shift from one paradigm to another therefore is not merely a psychological shift of *gestalts* or an unreasoned leap in the dark. Instead, it is a leap into the logic of narrative, a construction (or reconstruction) of a cognitive tradition in order to make sense anew.[34]

Positivists dismiss narrative and tradition and thus make themselves unaware of their own sources of intelligibility. By contrast, proponents of narrative and tradition respect the rights of theory and method but see them as subsumed within a larger social or historical context. Scientific customs—that is, traditions within scientific communities—govern such canons as falsification, the rules for linking observations with generalizations, and the degree and even the very definition of exactitude that may be required. When these customs compete, their respective intelligibilities, and the resolution of their antagonisms, are conducted through narrative discourse.

McCullagh's thorough and rigorous exposition of the positivist ontology and epistemology inadvertently reveals limitations that could be overcome by a broader concept of the nature and logic of the historical sciences. Narrative logic can also enrich the dialectic between positivism and idealism that Gellner develops. The subject matter and methods of both positivists and idealists are parts of two different stories about science, history, and society. Neither story provides direct access to reality, since each is part of the reality that it construes. In such a formulation, the subject-object dichotomy of the early debate between positivists and idealists is also transformed. One's own subjective states can be used as an analogic instrument for observing the subjective states of others. Such observations differ only in degree, not in kind, from "objective" observations; both types of observations are linguistically mediated, both are shaped by their instruments of observation, and both are subject in principle to validation by their coherence with a larger body of data and theory.

Positivist explanation and idealist interpretation need not be reductions of one to the other but can be transcodings from one form of representation into another. The positivist materialist would strip off the symbols to expose the underlying scientific reality, whereas the romantic idealist would dismiss scientific reality and intuit the true meaning of the symbols. But transcoding is an interpretation that becomes part of the subject matter under study, just as an interpretation of a poem becomes, on the next reading, a part of what the poem says. Both subject matter and explanation are symbolic. The interpretation—the study—becomes a metaphor for the symbolic expression studied. Each is deepened. Ways of doing

[34] MacIntyre, "Epistemological Crises," 69. Also see Richard M. Burian, "More Than a Marriage of Convenience: On the Inextricability of History and Philosophy of Science," *Philosophy of Science*, 44 (1977): 1–42.

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historical science may be judged by whether they build a wall between us and their objects or whether they make them newly accessible to us.

Such an analysis creates a space between personal agency as articulated through idealist interpretation and structural determinism as articulated through a positivist vocabulary. This space is the field for emancipatory theoretical practice. Deterministic theorists deny agency explicitly; subjective hermeneuticists vaporize it by making it impossible to find error in any group's version of the world. Explanatory scientists pose the question of conditions of the possibility of primary experience, thereby revealing that this experience, as understood in a hermeneutic phenomenology, is fundamentally defined as *not* posing this question. Whereas positivist scientists take their positivistic methods too seriously, idealist hermeneuticists take the "lifeworlds" of societal members for granted. But, from a critical perspective, emancipation is neither unrestricted agency nor unrestricted determinism; it is the overcoming of determinism by agency.[35]

Thus reformulated, the sterile dichotomy between positivism and romanticism becomes a fecund dialectic. A scientific critique of lay and proto-scientific ideas gives an emancipatory impulse to the historical sciences, provided that such a critique respects the existential significance of those ideas and does not itself make absolutist claims. Many phenomena in social existence are aggregative or emergent and thus require an explanation that transcends the categories used by the members of a particular culture. For such explanations to be critical and transformative, however, they must not become reified, must not lose contact with the lived realities from which they emerged. Stripped of its absolutist illusions, historical thought rediscovers its in-and-for-itself, its practical and emancipatory will to know.

[35] See Roy Bhaskar, "Emergence, Explanation, and Emancipation," in *Explaining Human Behavior, Consciousness, Human Action, and Social Structure*, Paul F. Secord, ed. (Beverly Hills, Calif., 1982); and Richard H. Brown, *Social Science as Civic Discourse: On the Invention, Legitimation and Uses of Social Theory* (Chicago, forthcoming).

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Reviews of Books

GENERAL

JAMES T. KLOPPENBERG. *Uncertain Victory: Social Democracy and Progressivism in European and American Thought, 1870–1920.* New York: Oxford University Press. 1986. Pp. x, 546. $39.95.

James T. Kloppenberg has written an ambitious and extraordinary book that merits wide attention from historians of both Europe and America. Any reader of this volume will quickly sense that here is a rigorous, first-rate mind at work, one that brings together a catholic range of scholarly interest with clean writing and magisterial judgments. This is not an easy or dilettantish volume; it speaks to a scholarly audience familiar with the issues involved. The first one hundred-fifty pages explore technical philosophical problems that will challenge many readers, perhaps even discourage some. But they will do well to persevere, for these unavoidably difficult pages add much to the overall argument and help give the book its special depth and originality.

The unusual scope of Kloppenberg's ambition is revealed in many areas, beyond his willingness and competence to delve deeply into traditional philosophical discourse. He has productively done what many others have only advocated: comparative history that significantly illuminates the national strands that he weaves together. He has attempted to show how, between 1870 and 1920, two generations of highly original thinkers ("philosophers of the via media") in Germany, France, Great Britain, and the United States broke away from long-standing traditions of either empiricism or idealism and, often unintentionally, met one another in a new arena of intellectual discourse. At the same time key political activists and theorists, whom Kloppenberg cautiously groups together as "progressives," made connection with these philosophers.

Kloppenberg does not conceive this connection simplistically; he does not present it in terms of ideas influencing one another in billiard-ball fashion but rather in terms of structural congruence, of moving into the same mental arena. These activist-theorists challenged the traditions of atomistic, possessive individualism, on the one hand, and socialism in its more dogmatic, collectivist, and revolutionary forms, on the other. Kloppenberg is not concerned with thinkers such as Freud and Nietzsche, although he does not deny their importance; rather, he calls attention "to those who acknowledged the importance of subjectivity and the limits of rationality, without turning to philosophies of force or succumbing to despair" (p. 10). Kloppenberg recognizes that we cannot ignore "the gulf of time separating their world from ours," yet he suggests that "it may be time to build bridges back to their ideas" (p. 11).

Such a bare outline of the nature and goals of this book can give only a pale sense of its subtlety, texture, and depth. Kloppenberg's claim to do comparative history worth the name lies partly in his solid mastery of the mushrooming secondary literature in four separate national fields. Moreover, even when he discusses intellectual matters, he does not remain narrowly focused on intellectual history as such. He has achieved a thorough command of the political, economic, and social history of the four countries concerned, and he puts this command to excellent use. The documentation in his endnotes constitutes nearly one hundred densely printed pages. Many of his notes are valuable historiographical essays in themselves.

Kloppenberg has read widely in primary sources, both published and archival, theoretical and private, again with an unusual sensitivity—and with a thoroughness that verges on the astonishing. I found his treatment of Max Weber particularly fascinating and provocative. He has also added significantly to my understanding of and esteem for John Dewey (admittedly in neither sense particularly profound), whom Kloppenberg presents as illustrating "most clearly the continuities of the philosophy of the via media and the politics of progressivism" (p. 299).

Comparative history of this sort, with four main national arenas and scores of other leitmotifs, faces an obvious danger of repetitiveness, and Klop-

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penberg does not always escape that danger. Similarly, the opening pages attempt what may be an impossible task, that is, to present ideas in brief that stubbornly resist condensation (Kantian ethics and epistemology, for example). At times, I felt that Kloppenberg tried to pack too much into these pages, particularly by including relatively minor figures and pursuing rarified nuances, thus overburdening a text already richly laden with major themes. Even though he normally writes with rare clarity and grace, on occasion Kloppenberg slips into the use of easy labels or into tangled constructions. "As Bernstein understood, Marx hitched Kant's antimonies to the dynamism of historical change by appropriating Hegel's dialectic, and he sought to reconcile them through praxis. Yet Marx's epistemology and politics ultimately rested on the ideal of man as species-being, a concept of both ethical and political dimensions that indirectly restated Kant's categorical imperative and again pointed to the coherence of Marx's thought with an inverted ideal of the *Rechtsstaat*" (p. 220).

But these minor cavils should not significantly detract from well-deserved praise for an ambitious, challenging, and successful work.

ALBERT S. LINDEMANN
University of California, Santa Barbara

JAMES DENARDO. *Power in Numbers: The Political Strategy of Protest and Rebellion*. Princeton: Princeton University Press. 1985. Pp. xvi, 267. $25.00.

In a difficult theoretical work James DeNardo postulates a number of models of political behavior that he hopes will clarify the relationships between radical movements and their opponents. The underpinning of this discussion is a derivative form of microeconomic theory: most particularly indifference theory. As the argument is developed, historical examples are introduced to illustrate and confirm the models.

Certain questions come to mind. Do the theoretical models DeNardo describes conform to the reality of revolutionary experience? Can such models either serve to explain historical episodes or, if applied to contemporary events, predict the future course of revolutionary movements? The author's models only partially fulfill the first and second and are of doubtful utility for the third of these questions.

The structure of the argument is built on an initial limiting factor: DeNardo does not presume to "explain why strikes, rebellions, and civil disorders occur when and where they do" (p. 6). Instead, the author tries to show that the planning and strategy that revolutionary leaders use has a certain logic and impels such leaders to pursue goals that would maximize their strategic desires. The careers of V. I. Lenin, Leon Trotsky, and Julius Martov, as well as the famous debate between Eduard Bernstein and Karl Kautsky, furnish evidence to confirm the contention that "purist" revolutionary ideologues contend in the political arena with "pragmatists" as well as with "moderates." The intransigence of incumbent regimes is given as a known factor, although the degree of such intransigence fluctuates according to the use of strategic options by "dissidents" who oppose these regimes. Added to the system is the factor of mass involvement. For example, if revolutionary leaders pursue a nonviolent course a certain number of adherents would be gained by them, but only modest results could be obtained from the recalcitrant authorities. Resort to radical strategies splits the dissidents into "purists" who, like Lenin, wish to preserve the ideological purity of aims through total conquest of the regime and "pragmatists" who shrink from such an option and seek accommodation with the regime. Models are provided that illustrate, with indifference techniques, the range wherein broad support for various strategies can be obtained. Certain assumptions punctuate the argument: "People who join demonstrations must also face up to the personal consequences of suffering repression" (p. 193), and "To pursue an optimal strategy, the dissidents must locate the most effective tactic against every policy the government might adopt" (p. 203).

This argument is most striking for its connection to the famous ideological debates between Marxists from 1890 until the Russian revolution. The highly developed tactical posture of Lenin fits beautifully into the author's system of models. Like his models, the logic of the different positions adopted by Marxist intellectuals led to confrontations between them. The bloodless battles of leftist intellectuals lasted years, underwent many permutations, and caused the solidification of radical, émigré parties. The models are beautiful intellectual creations and work very well in elucidating these debates.

What does all of this have to do with actual revolutions? Unfortunately, not very much, because the models are not very applicable to the confused and erratic course of immense revolutionary events. The Russian revolution was not caused by Lenin, Martov, or Trotsky; they entered on its stage at particular points. Did Lenin in 1917 faithfully carry out his strategies of 1903? If the models are applied to the French revolution they make even less sense. That revolution was not caused and led by rational people who constantly referred to a rigid calculus of options. Ideological debate may well have been less important than the identification of politicians with certain actions and with other politicians of a particular type. In the French revolution a real difference

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separated mass actions, including impromptu parades, demonstrations, and riots, from the staged and planned activities of radical politicians. Missing in DeNardo's scheme is the inchoate mass of people who participate, off and on, in revolutions for a variety of private, even bizarre, reasons, as well as for avowedly political ones.

Of course, the book is devoted to the strategy of political leaders and has validity because it clarifies the thought processes of the theorists of revolution even if it is lacking in understanding of the practitioners of revolution.

HUBERT C. JOHNSON
University of Saskatchewan

HANS-JÜRGEN GOERTZ, editor. *Alles gehört allen: Das Experiment Gütergemeinschaft vom 16. Jahrhundert bis heute*. (Beck'sche Schwarze Reihe, number 289.) Munich: C. H. Beck. 1984. Pp. 245.

The nine essays plus Hans-Jürgen Goertz's introduction gathered in this collection are a reminder that communalism is a phenomenon that will not disappear from social systems that exalt private property and the competitive individualism needed to get it and hold on to it. Although never more than a marginal feature in the development of the West, communalism has not lacked attention from historians and social scientists. And no wonder: the moral fervor impelling all such experiments in organized property sharing is a bracing thing to experience when we meet it in the many utopias of early modern, modern, and recent European and American history. Whether religious or secular in origin, ethical collectivism is the appeal of conscience against mutual rivalry and exploitation. The examples of it discussed in this book offer many occasions for sneering at the experimenters' naive idealism, and naiveté there certainly was in the responses to their respective conditions made by Hutterites, rural egalitarians such as Michael Gaismair and Gerrard Winstanley, the Doukhobars of eighteenth-century Russia, Pietists such as Johann Conrad Beissel and Johann Georg Rapp, industrial socialists such as Alexandre Dézamy in France and Wilhelm Christian Weitling in Germany, and "Green" youth groups leaving the demoralized cities of Germany after World War I for romantic communes in the countryside. But it was the naiveté of conscious innocence, which was the posture of alienated men and women casting a fresh and uncomplicitous look at their world and setting out to act on the changed perspective. There is much to admire in the—mostly failed—attempts at creating alternative social structures. More important: there is a good deal to learn from them that could be useful to us as correctives.

Faith imparted the momentum. But what is most interesting to the historian is the confluence of evangelical inspiration and alienating circumstances that is revealed in the better essays in this volume (James Stayer's on Anabaptists, Klaus Deppermann's on the Diggers, Klaus Hansen's on Ephrata, Harmony, and Oneida in the United States) to have moved founders and organizers to action. Crisis always gave the impetus; unsurprisingly, therefore, most egalitarian endeavors date from the sixteenth and seventeenth centuries, the age of the Reformation, and the first half of the nineteenth, the time of early industrialism. The common denominator was revulsion against capitalist values as perceived, and sometimes experienced, by victims and outsiders. Some of them reacted with their heads—Winstanley in his *Law of Freedom* (1652), Weitling in *Humanity: As It Is and As It Should Be* (1838)—others with their feet: they joined the German *Siedlung* movement in the 1920s and 1930s, Brook Farm or the North American Phalanx in America, or the Hutterite *Bruderhöfe* of the dispersed Anabaptists. The object, always, was to disprove, by living witness, what Hansen, writing on North American communes, calls "the tyranny of John Locke": the argument that private property is the surest guarantee of life and liberty in organized society. A few among those who found this contention unacceptable on moral grounds, and whose lives had been touched by its consequences, took the road to separation. The choice they made reflects not only their rejection of the dominant society but also their acknowledgment of its power. Joining it was wicked. Fighting it was useless. Turning one's back on it seemed the only way. This book, written for the general reader (perhaps the German reader puzzled by the vogue for "alternatives" among so many young Germans today), tells their story.

GERALD STRAUSS
Indiana University

CHARLES WHITNEY. *Francis Bacon and Modernity*. New Haven: Yale University Press. 1986. Pp. x, 234. $18.50.

Charles Whitney writes about Bacon to clarify the "modernity" of the modern age. He claims that a proper grasp of Bacon's proposals for the scientific conquest of nature will cure the naive optimism that views modernity as the project of pragmatic rationality, fully self-knowing and wholly independent of any "secularized" premodern or religious (Christian) roots. Such optimism appears in Hans Blumenberg's treatment of Bacon in *The Legitimacy of the Modern Age* (1983) and in the interpretation of Bacon's *Essays* in Stanley Fish's *Self-Consuming Artifacts* (1972). According to Whitney, Blumenberg

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wrongly tries to separate Bacon's dependence on the rhetorical and prophetic traditions from a genuinely modern core of ideas, and Fish wrongly tries to explain the *Essays*' debunking of its traditional topics as the result of Bacon's self-consciously revolutionary intention. For Whitney Bacon's project is neither a revolution clothed in some traditinal garb nor a clever assault by a radical founder on an old world. Rather, its modernity consists in the tensions between old and new, traditional and revolutionary, and between sociolinguistic determinism and free self-assertion.

Whitney makes his case by discovering a twofold structure of Bacon's project for the complete reformation of learning—the Great Instauration—as it emerges from his major works. In the works, and hence in the project, we find two contending senses of *instauratio*, one denoting reform and building from the traditions of prophecy, Ciceronian rhetoric, and Renaissance humanism and the other denoting the revolutionary rejection of all interpretive traditions in favor of the direct, scientific encounter with nature's truth, the mastering of its powers, and the readying of humanity for encountering the new and the strange. For Whitney Bacon is quintessentially modern because his attempt to free the mind and the truth from all traditions always echoes some tradition and because, since the stance of modernity consists in an iron-willed readiness for the new, the echo cannot be heard at its source (except by English professors who know about ideology and deconstruction). For Whitney Bacon's modernity is a "tragedy of meaning" because its characteristic articulation of radical human independence cannot occur apart from an inherited and determining web of discourse and interpretation.

Whitney's Bacon exposes the ultimate limits of every attempt to do what is perhaps characteristically modern: to define ourselves as moderns, located in a present moment grounded solely on free self-assertion. Thus, Whitney argues that from Bacon we learn why it is now thought that every such definition can be deconstructed and so unmasked as an ideology defending some present domination inherited from the past. But even so, says Whitney, we can find in Bacon's modernity something akin to a "metadiscourse" that defies the historicist skepticism of contemporary deconstruction, a skepticism Whitney fears encourages conservative resignation in the face of powers that always will be. Thus, despite its instrumentalism and will to domination, Bacon's project for science is not simply bourgeois ideology. Rather, its pursuit of novelty is always critical and open to a "radically nonauthoritarian, socialist political engagement" (p. 125) such as is expressed in Jürgen Habermas's vision of rational, universalized discourse. Modernity is tragic. But not to worry: so long as we are true Baconians—that is, critical egalitarians—modern life will always be brightened by a silver lining.

Whitney is at his best when he argues that the foundationalism of Bacon's method is a demand not for incorrigibly certain knowledge but rather for the negation of every mediation between mind and the truths of nature. He falters, however, when he explains the Baconian tension between old and new in Freudian terms, arguing that it is an example of the symbolic—and so, culture-preserving—repetition of original parricide. This brief flight of fancy mars what is otherwise a fine book. I am unconvinced by most of Whitney's arguments. But the book is lively and interesting and sure to spark helpful controversy. And Whitney has done all a favor by reminding us that, whether we intend to criticize or legitimize the modern age, we must come to grips with Bacon. Although cast in the language and concerns of modern literary criticism, this book should be of interest to intellectual historians and all who ponder the questions of secularization and the meaning of the modern age.

JERRY WEINBERGER
Michigan State University

KENNETH W. THOMPSON. *Toynbee's Philosophy of World History and Politics*. (Political Traditions in Foreign Policy.) Baton Rouge: Louisiana State University Press. 1985. Pp. 230. $27.50.

The time is coming, and may be at hand, for a reappraisal of Arnold J. Toynbee. All of the works of this most famous historian of the twentieth century are now published, along with his correspondence with an English Benedictine monk, Father Columba, over nearly forty years, letters that reveal the religious secret within his histories. Enough time has lapsed since the Toynbee whirlwind of the 1950s for us to look calmly at his legacy.

Thus, we turn eagerly to Kenneth W. Thompson's book because he is probably the commentator best suited to examine a theme not treated fully before, Toynbee's views on international affairs. Thompson reminds us that Toynbee's special field as a historian, besides Greece, was international affairs and that Toynbee himself produced the monumental annual series *Survey of International Affairs* during the 1920s and 1930s.

Thompson's leading theses are two. First, Toynbee did have a theory of international relations, albeit indirectly expressed via his historical analysis, which led him to identify what he regarded to be laws or timeless principles of international affairs. Second, in enunciating his theory, Toynbee moved from a more or less "idealist" phase to a more or less "realist" phase in which he eschewed moralisms and emphasized the hard reality of the

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struggle for power among nations as did the "old diplomacy" of eighteenth-century diplomats, Metternich, and Bismarck. Thompson's first thesis is well made. In making it, however, he often gives us more Thompson than Toynbee, using Toynbee as a resource for his own points. Consequently, in depicting Toynbee as a convert to his own "realist" school, Thompson overlooks the evident mixture of ethical imperative and critical analysis of power that Toynbee cultivated throughout his career.

The book suffers from a deeper problem. The content and notes reveal that Thompson does not use for his study any of Toynbee's works published after 1949, works that continued to appear until 1981. He omits—to name only the most notable—volumes 7 to 10 of *A Study of History* (1954), *An Historian's Approach to Religion* (1956), *Reconsiderations* (1961), *America and the World Revolution* (1962), *Experiences* (1969), and the one-volume revision of *Study* (1972). In these volumes Toynbee gave his best statements of his views on universal states, contacts between civilizations and between states, U.S.-Soviet relations, religion, the prospects of Western civilization, law and freedom, and historical method, and he identified the sources of his thought and the radical changes he wished to adopt in his overall theory of civilizations. Thompson has covered all these themes but without using any of these most crucial texts. The puzzle created by such extraordinary omissions is compounded when we note a detail not included in the book: that most of the material of four out of Thompson's seven chapters was published in 1955 and 1956 in five articles, which even then did not use Toynbee's major statements in volumes 7 to 10 of *Study* (1954). The book could have solved the puzzle for us by citing Thompson's doctoral dissertation of 1951, written under the direction of the "realist" Hans J. Morgenthau, entitled "The Philosophy of International Relations of Arnold J. Toynbee." The book before us today was essentially thirty-five years old when it appeared. That Thompson has missed the opportunity to give us a book on the whole of Toynbee's protean thought on international affairs is our loss.

C. T. MCINTIRE
University of Toronto

CLARENCE J. KARIER. *Scientists of the Mind: Intellectual Founders of Modern Psychology*. Urbana: University of Illinois Press. 1986. Pp. xi, 356. $27.50.

GERHARD WEHR. *Carl Gustav Jung: Leben, Werk, Wirkung*. Munich: Kösel. 1985. Pp. 478. DM 48.

Twentieth-century Western psychological theories and schools have achieved sufficient longevity and influence to prompt scholars in diverse fields to re-examine traditional assessments of their significance. The two books under review, one by a specialist in educational policy and the other by a scholar of religious mysticism, are examples of that reassessment.

Clarence J. Karier's *Scientists of the Mind* devotes chapters to William James, G. H. Mead, E. L. Thorndike, G. S. Hall, J. B. Watson, Sigmund Freud, Alfred Adler, C. G. Jung, Margaret Naumberg and Waldo Frank, and Otto Rank. Each chapter, except for the one on Naumberg and Frank, combines intellectual history and psychohistory in the service of Karier's main thesis: modern psychologies carry the religious and philosophical views of their founders and are as well the thinkers' solutions to their idiosyncratic maturational problems. Karier further argues that each psychological approach is a reflection of the shift of Western society in the nineteenth century from a world centered on God to a world centered on humanity. This metamorphosis played havoc with traditional explanations of human behavior and created a variety of existential crises, which are still extant. For some reason, Karier believes the philosophical assumptions and the social meanings of the new psychological systems have remained largely unexamined. Therefore, he states that Western peoples have detrimentally accepted or acted on theories of whose significance they have been unaware; the purpose of his book is to remedy the situation.

In analyzing his chosen psychologies, Karier finds that their originators were guilty of supporting racist, sexist, or elitist-establishment biases. In addition, James, Mead, Thorndike, Watson, Freud, and Jung carried over their religious beliefs into their psychologies; Hall, Freud, Adler, and Jung created religions in disguise; and Watson, Hall, Jung, and Rank were irrationalists who rejected "Enlightenment . . . principles of science, reason, freedom, and rational understanding" (p. 341). I have not mentioned Naumberg and Frank because I do not think they belong to the group labeled by Karier as "intellectual founders of modern psychology."

Persons familiar with nineteenth-century Western history and the above-mentioned psychologists' lives and careers will not feel their time has been well spent reading *Scientists of the Mind*—and not only because the material is not new. Certainly, well-known topics can be discussed in a way that enriches the reader's understanding of them. But to do so authors have to be steeped in their subjects, which is obviously not the case with most of the psychologists Karier discusses. The impetus behind the book is Karier's pain in living in today's world, which he finds "fascist," antirationalist, and alienating. This motivation is insufficient by itself to support an original, scholarly work. The book also suffers from an annoyingly repetitive prose style, a murky intro-

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duction, the frequent lack of up-to-date sources, and a vitriolic attack against J. B. Watson. Watson is no hero of mine, but Karier's "analysis" is an unfortunate abuse of the psychohistorical method.

Karier concludes that only a metaphysician, who "has not yet arrived," can be the "Galileo of psychology" (p. 343). To read Gerhard Wehr's biography, *Carl Gustav Jung*, however, one would think this philosopher-king had already come. According to Wehr, Jung's analytical psychology is increasingly recognized as offering individuals the way to the realization of their true natures.

Unlike Karier, Wehr is a master of his subject, and his book is well grounded in Jung's letters and memoirs and in an extensive secondary literature. There is much to be learned or reminded of here. But the work is not the definitive biography the publisher claims. It is written primarily as an apologia for Jung rather than as an even-handed appraisal. Two examples must suffice. Jung's open support of National Socialism and "Aryan psychology," which has stirred great controversy over the years, is accepted as the "dark side" of Jung's personality and discussed in the most generous possible terms. And Martin Buber's analysis of Jung's gnosticism in *The Eclipse of God* (1952) is not accorded serious consideration but rather is explained away as Buber's failure to understand what Jung really meant by the "actuality of the soul" and the process of individuation (*Ganzwerdung*). In general, Wehr's defense of Jung rests heavily on his use of Jung's repeated complaint that he was not understood.

The relationship between Jung and Freud is evaluated fairly accurately. Wehr perceptively sees that the issue of the importance of sexuality was not central to the break between the two men. Jung had made clear to Freud at the start of their relationship that his acceptance of Freud's emphasis on sexuality was qualified. Rather, Jung began to pull away from Freud after 1909 because of his growing interest in the mythic, which eventually led him to his theory of archetypes as the basic elements of the psyche. Indeed, Wehr's biography demonstrates that Jung's encounter with Freud, though crucial, was only one aspect of his career. Jung's concern with spiritual matters was decisive throughout his life and overrode any single influence. Although this interpretation of Jung has been put forth previously, it bears restating. Wehr's conception of Jung is thus correct in many essentials. Nevertheless, his wider assessments cannot stand as definitive. Neither Wehr's exaltation of Jung nor Karier's unrealistic expectations of modern psychology have led to satisfying works.

HANNAH S. DECKER
University of Houston

1. BERNARD COHEN. *Revolution in Science*. Cambridge: Belknap Press of Harvard University Press. 1985. Pp. xx, 711. $25.00.

I. Bernard Cohen is a distinguished historian of science and an acknowledged authority on Newton, to whom he has devoted an impressive output of books and articles in the course of a long career. The appearance of this massive volume is an important event in the history of science and has been awaited by scholars since the author promised it in his *The Newtonian Revolution* (1980), to which the present work may be seen as a sequel on a much larger scale.

Cohen states, "This work is part of a broad research program with a double aim. In part I am concerned with exploring and elucidating the creative process by which a practitioner in one discipline uses the ideas . . . of another discipline. . . . The second goal of my research is to define and analyze the interactions between the natural and exact sciences and the social and behavioural sciences. . . . Case histories of particular revolutions . . . make up a large part of the book" (pp. xiii–xiv). Cohen offers a pleasing blend of theory and practice: "This book treats not only of the concept of revolution but displays some of the major features of the actual revolutions in science" (p. 25).

First Cohen establishes that "revolution" is a suitable word to describe a breakthrough in science and that it is not merely a modern term, for in the past three centuries scientists and their contemporaries spoke of their work in this way. Naturally, Cohen acknowledges T. S. Kuhn's influential work, *The Structure of Scientific Revolutions* (1962), but this is not allowed to dominate the discussion. Cohen posits four stages leading to a revolution in science: first, an "intellectual revolution" or "revolution in itself," that is, the initial creative act of an individual scientist; second, a "revolution of commitment" of the scientist to the new method, concept, or theory; third, a "revolution on paper," or full working out and public dissemination of the new idea; and fourth, the "scientific revolution" itself, when "a sufficient number of other scientists become convinced and . . . begin to do their science in the revolutionary new way" (pp. 28–31).

Then he considers, with many examples, how a scientific revolution can be recognized, and he establishes four "purely historical and factual" tests "that may be universally applied to all major scientific events that have occurred during the past four centuries" (p. 41): first, the testimony of contemporary witnesses that the new development is or will lead to a revolution; second, the evidence of adoption of the new features in the subsequent history of the subject; third, the judgment of competent historians; fourth, the general opinion of working scien-

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tists in the field today (pp. 41–47). This may seem to smack of hindsight; however, it is a serious attempt to secure objective criteria for recognizing a breakthrough in science.

Cohen examines the concept of revolution in its political (and also its astronomical) meaning from classical times, through the Middle Ages and the Renaissance, down to the national convulsions of the seventeenth and eighteenth centuries, and he speculates that "the revolutionary spirit which moved in the realm of politics might well have been the same as that which caused upheavals in the sciences" (p. 78). From politics he moves to the scientific revolutions of the sixteenth and seventeenth centuries, and he investigates, by means of the above-mentioned four tests, which of the scientific developments of that period deserve to be called revolutions. Nicolaus Copernicus, Johannes Kepler, William Gilbert, Galileo, Francis Bacon, Andreas Vesalius, and Theophrastus Paracelsus were revolutionary thinkers, but their work did not, he decides, amount to a revolution in science. Only René Descartes, William Harvey, and Isaac Newton really made revolutions, and of these Newton's was incomparably the greatest: "The Newtonian revolution was not only the apex of the Scientific Revolution, it remains one of the most profound revolutions in the history of thought" (p. 175).

Cohen attempts to show that further scientific revolutions occurred on the public and institutional level in the nineteenth and twentieth centuries, with the advent of professionalism in science, new journals and societies, scientific departments in universities, and governmental involvement and expenditure on science. Here, however, he is less than convincing. He is on more solid ground when he deals with the acknowledged revolutions in nineteenth-century science. First and foremost, of course, was the Darwinian revolution, whose "tremendous revolutionary impact . . . arose in some measure from the extra-scientific component . . . the concomitant ideological revolution" (pp. 283–84). Less famous but perhaps more profound was the revolution in physics brought about by the work of James Clerk Maxwell. Cohen also deals with nineteenth-century revolutions in geology, physiology, mathematics, and technology and discusses the influence of Marx and Engels, especially with respect to Darwinianism.

On the border of the nineteenth and twentieth centuries stands the Freudian revolution. Then, in the twentieth century, revolutions in science come thick and fast. Predominant, of course, is the revolution caused by Einstein's work on relativity, although Einstein himself preferred to think of his work as evolutionary rather than revolutionary. Cohen also deals with quantum theory as a revolution in physics and with plate tectonics as a revolution in earth science. Surprisingly, he has little to say about molecular biology, surely one of the most revolutionary twentieth-century developments in the life sciences, about medicine after the seventeenth century, and about computer science. Many will feel that these are serious omissions.

Where so much is offered to the reader, it is perhaps unfair to cavil at omissions. This book covers an enormous field. Dealing with scientific developments over four centuries, it presents the reflections of the scientists themselves, and their contemporaries, on the work they were undertaking. It draws attention to the ideological repercussions of revolutionary scientific ideas and their impact on current attitudes. Cohen adds to the general understanding of his theme by his examination of language, of the historiography of science, and—although it merits, as he admits, a more thorough exploration than he gives it—of "the interaction between the concepts of political or social revolution and revolution in science" (p. xiv).

This book will be a must for historians of science. Other historians, too, especially those concerned with the history of ideas and with the study of political and social revolutions, will find much to interest them here. They need not be afraid that they will be vexed by scientific jargon. Cohen writes interestingly and clearly, and the strongest feature of the book is the wealth of concrete examples that illuminate his argument. In his own words, "Many readers will find the case histories of particular revolutions to be of greatest interest" (p. xiv).

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KENNETH J. CARPENTER. *The History of Scurvy and Vitamin C.* New York: Cambridge University Press. 1986. Pp. viii, 288. $39.50.

That Kenneth J. Carpenter, a professor of nutrition, is also a fine historian is made evident in this superbly crafted study of a disease that has caused untold millions to die over the centuries yet eluded attempts to master it until well into the present century.

The work begins with an exhaustive revelation of scurvy in the records of the important voyages of exploration from 1498 to 1700, pauses to examine how scholars viewed the disease during these years, moves forward to consider the problems the disease posed for the British navy during the eighteenth century (in the process providing a balanced account of James Lind's contributions), and then shows how the pragmatic methods of Captain Cook for keeping the disease at bay were almost lost amid medical theorizing. The result was that the Admiralty finally swept the disease from the ships by the

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regular issuance of lemon juice to the men only at the end of the century.

For the nineteenth century Carpenter leaves the sea to consider the disease on land: in prisons, during the great Potato Famine, among the "Forty-niners" in California, and among the soldiers that clashed in the Crimea and the American Civil War, as well as the civilians who suffered through the siege of Paris during the Franco-Prussian War. Clearly, the success of the British navy in preventing "sea scurvy" eluded those battling "land scurvy," and nowhere was this struggle more fascinating than in the Arctic, where the experience of its scurvy-plagued explorers is contrasted with that of the scurvy-free Eskimos.

Toward the close of the nineteenth century scurvy appeared on an unexpected front, among infants, often of the well-to-do, as bottle rather than breast feeding became fashionable. But why? To this point the historian has told the story of scurvy, but the nutritionist has been holding out on the reader, who may have guessed that potatoes contain enough vitamin C that their scarcity during the potato famine brought on scurvy. But why were those Eskimos free of scurvy, and why was lime juice not as effective as lemon juice, and why were neither effective at times, and what about those infants?

These and a number of other questions are resolved in the last portion of the study as the nutritionist gradually takes over. Carpenter begins by revealing the trial-and-error experiments of the twentieth century that led to the discovery that guinea pigs could serve as human models and, subsequently, in 1935, to the discovery of vitamin C. He follows this with a chapter on the modern knowledge that has accumulated on the vitamin and considers the debate on the efficacy of large doses in disease prevention and therapy. He concludes with a retrospective chapter that clears up most questions that might still be nagging at the reader. But not all questions are answered, for some remain inexplicable. For example, why have past "cures" for the disease that should not have worked nevertheless been effective, and why do some infants (such as Bantu infants studied in South Africa) appear immune to the disease although their parents are not. And, finally, did Captain Robert Scott and his companions at the South Pole die of scurvy?

Save for a technical chapter on the needs and uses for vitamin C from 1935 to 1985, nonspecialists will have no difficulty in following Carpenter through this fascinating story, and they will find the text illuminating, the close to twenty-five pages of bibliographic citations awesome, and the well-done index extremely useful.

Some (like myself) will be disappointed that the author did not devote a chapter to the slave trade in which the disease is thought to have been an important killer for centuries. But this omission in no way detracts from the importance of this study, which makes a major contribution to the growing body of literature devoted to demonstrating the crucial significance of the biological dimension to the history of humankind.

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STEPHANIE COONTZ and PETA HENDERSON, editors. *Women's Work, Men's Property: The Origins of Gender and Class*. London: Verso; distributed by Schocken, New York. 1986. Pp. 220. Cloth $28.00, paper $8.95.

Many respectable scholars think that academic works with an obvious political purpose are not worthy of serious consideration; others hold similar opinions about Marxist works. They need read no further: this is not the book that will change their minds. I review the book for the benefit of Marxist feminists and those who take an interest in their literature.

The book is a collection of essays "on the development of sex role differentiation and sexual inequality" (p. 1), written chiefly by the editors, Stephanie Coontz and Peta Henderson, and by their French co-workers, Nicole Chevillard and Sebastien Leconte. The authors, confronting the fact of almost universal male dominance (in the Marxist sense of controlling the surplus value of women's labor) and refusing to consider this "natural," attempt to describe the origins and triumph of male dominance. After a long introduction by the editors, reviewing earlier work in particularly dismissive fashion (the subtitle is "'Explanations' of Male Dominance"), Lila Leibowitz describes the rise of role differentiation—not yet inequality—as humanity arose from its putative primate ancestors. A pair of articles describe the emergence of kinship societies and the supersession of the matrilineal by the patrilineal and patriarchal clan, either by violence (Chevillard and Leconte) or evolution (Coontz and Henderson). A short essay by Chevillard and Leconte argues that the status of female citizens was little different from that of slaves, and the last contribution, by Monique Saliou, argues that the rise of the state in ancient Greece hardened the oppression of women.

Politics has preceded scholarship, and the authors make little effort to hide the fact. The authors state that "eradicating the oppression and exploitation of women is [a] worthwhile . . . goal" (p. 75). If male dominance is natural—even a historical development from biological differences—then it is presumably ineradicable; but, if it is a product of historical factors, it may be reversible. The authors pay no

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compliment to their self-awareness when they purport, on page 35, to find it "striking" that they agree on the second point of view. Granting that strong political opinions do not disqualify an author from writing a good scholarly work, one has reservations about history that occurred precisely in the way that politics require.

Disdain for the opinions of non-Marxists and nonfeminists does not help the editors' case: their own demonstrations are hardly iron-clad, but others' are rejected by the mere raising of doubts, a double standard that will convert only the converted.

Beyond these objections, though, is a deeper difficulty. This book is neither history nor anthropology. History interprets the remnants, anthropology the current state of real and known societies. *Women's Work* deals almost entirely with hypothetical, and perhaps imaginary, societies—with "growing up hominid" (p. 55) and "Neolithic kin corporate societies" (p. 126). Evidence about these societies varies from nothing at all to precious little. Archaeology has its limits; the authors instead take examples from modern "primitive" societies, described by sweeping generalizations (a revealing note on page 117 shows how much these descriptions may be in the mind of the describer), and then depict one sort of society developing into another. (The last essay deals similarly with myths.) This is not only unprovable but entirely arbitrary. Extrapolation backward into an unknown past has no more likelihood of accuracy than extrapolation forward into an unknown future. It is not history; it is mythmaking.

The book is not ill-produced, and the prose no worse than normal academic jargon. A classicist could have told the translator of the last French essay that "Denys" is Dionysius in English, "Locres" Locri, an "epiclere" an *epikleros* (pp. 186–88); an "Alcmaeonide" has no final *e*, but "Achille" does have an *s* (the Phaeacians, though, are hypercorrected to "Pheatians" on page 183). These are minor illiteracies in a book whose real problems lie deeper and did not all originate with the authors.

DAVID M. SCHAPS
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THEDA SKOCPOL, editor. *Vision and Method in Historical Sociology*. New York: Cambridge University Press. 1984. Pp. xiii, 410. Cloth $39.50, paper $13.95.

In Weimar Germany (and probably elsewhere) it was often remarked that students of sociology were too dim for philosophy and too lazy for history. If this was so then, much has changed, as this volume, edited by Theda Skocpol, testifies. In 1979 a group of sociologists, reared in the discipline during the later 1960s, convened in Cambridge, Massachusetts, to hold a conference on methods of historical social analysis. About the same time, a much larger contingent within the American Sociological Association began agitating for the establishment of a "Comparative-Historical" section of the guild. After a long period of ahistorical theorizing and research, American sociology has rushed back to history, where it began, like a child to its rediscovered mother. This volume is one of many that have appeared recently, spelling out in theoretical or practical terms what sociologists need to do when using historical materials. Yet, unlike other books, Skocpol's collection is distinct in its organization and purpose. Of the eleven scholars who contributed chapters, all but one earned their doctorates after the mid-1970s. Each assumes responsibility for outlining the achievements and failures, small though they might be, of nine senior scholars whose work or person influenced their own: Marc Bloch, Karl Polanyi, S. N. Eisenstadt, Reinhard Bendix, Perry Anderson, E. P. Thompson, Charles Tilly, Immanuel Wallerstein, and Barrington Moore. In chapters of roughly equal length, each man is treated to a sympathetic commentary, sometimes a short biography, and then subjected to mild critique in terms of whatever research program or epistemology speaks to the younger scholar's interests. Each chapter also ends with a small bibliography of the subject's works in English and a list of important reviews. The editor has supplied an introduction, conclusion, and annotated bibliography on methods, and the book closes not with an index but with the longest notes on contributors ever to appear in a scholarly volume, so far as I know (pp. 404–10).

The quality of writing and thinking does not vary so much among the chapters of this collection as is often the case. Attitudes toward the subject in question, however, run from hearty praise to gentle scorn. Some characterizations of the author or work under scrutiny do not coincide with my own reading, but that probably reflects legitimate hermeneutic differences rather than willful distortion. In other words, the contributors employ no apparent program or, to use Tilly's favorite word, "agenda" to belittle senior authors and benefit their students. Skocpol's own preference, tabularly expressed in her conclusion, is for the "analytic historical sociology" she learned from Moore, rather than the application of general models to historical data (as in the work of Neil Smelser, Wallerstein, and Tilly) or the creation of "interpretive historical sociology" that relies heavily on aesthetically satisfying reconstructions of events or Zeitgeists and plays down nomothetic regularity (as proposed by Clifford Geertz, Bendix, Thompson, and Anderson). But she is careful to give the alternative styles their due

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and not allow her pursuit of "causal regularities" to blind her to the strengths of other approaches. Because of this even-handedness throughout, the book might well serve as a trustworthy guide for graduate students just beginning to read these authors and, at points, will also stimulate older scholars to reconsider their appraisals of some classics.

Since evaluating each chapter is not possible here, some general observations may help locate the volume at the nexus of history and sociology (or social science generally, since the authors in question and their students also use political science, economics, even psychology and anthropology). The least sympathetic chapter is Dietrich Rueschmeyer's on Bendix. His dislike of Bendix's "anticipatory skepticism"—his refusal to practice scientistic history—culminates with the suggestions that "testing of explicated theorems and their modification in continued use" (p. 159) in order "to bypass the subjective dimension and focus, for instance, on structural features of social life" (p. 160) is a preferable strategy. This sort of complaint—that the older writers dealt overmuch with the specific "richness" of a locale, period, mode of production, nation, or set of culturally motivating ideas—arises repeatedly in various forms. The new historical sociology seems to fear narrative pleasures ("a surrender to pure idiography" [p. 286]) as much as it needs the "data" within them. Ellen Kay Trimberger's appreciation of Thompson's "interpretive argument" and "dialectical insights" (p. 231) is the least concerned with large-scale or even middle-scale regularities of historical experience and approves of Thompson's idiography of English labor. Yet most of her colleagues seem somewhat put out by this type of historiography. No matter how much they admire Bloch, Bendix, Thompson, or Anderson—for their prose, storytelling, and narrative structuring of the past—they seem more comfortable with the hypothesis testing of Tilly or the counterfactual "experiments" of Moore. After all, if sociology is to stand beside history and not be swallowed by it, "regularities" that are otherwise missed must be identified. The contributors also seem to resist the political motivations behind the work of Wallerstein and Anderson, as if to say that today's historical sociology will purge itself of political obligations in the interest of "scientific" advance.

I have one last point: virtually every major comparativist of the past was a linguist (Max Weber used eleven languages), and younger American sociologists are uniformly not. Skocpol argues (p. 382) that one can now rely on "excellent" secondary sources in pinning down large-scale regularities, which may be true, but neither she nor her colleagues ask the more serious question of how we are to compare anything without knowledge of foreign languages. Perhaps in the future graduate students in sociology will be allowed to substitute Chinese or German for computer languages.

ALAN SICA
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THOMAS L. HASKELL, editor. *The Authority of Experts: Studies in History and Theory.* (Interdisciplinary Studies in History.) Bloomington: Indiana University Press. 1984. Pp. xxxix, 278. $22.50.

Reading this series of interdisciplinary essays on the role of experts and the professions in American society, I found myself a participant in the dialogue being attempted. At first the book appears scattered, confused, and without clear direction. Each of the essays seemed to have some merit, but the collection appeared to lack coherent focus. Closer reading, however, began to reveal that the apparent unconnectedness is in fact a demonstration of the state of the question. Eliot Freidson, whose essay sets the tone of the debate and poses its central questions, states boldly, "I think it is fair to say that scholarship concerned with the professions is in an intellectual shambles" (p. 5). Freidson attempts a definition of the basics of professions and poses three criteria: expertise, establishing credentials, and occupational autonomy. He then confronts the abolitionist school, in particular Ivan Illich, who attack the notion of credentials or (in Illich's case) the idea of occupational expertise itself.

The remainder of the essays were written by historians, sociologists, a psychologist, a philosopher, and a legal scholar, and they explore theories of the professions and historical cases. Among these should be mentioned Sanford Levinson's "Law as Literature: Do Legal Texts have Authoritative Interpretations?" which ends with an attempt to wrestle with the ethical dilemma between commitment and objectivity. In view of the controversies of the moment, his analysis of the criticisms of the place of Justice William Rehnquist on the Supreme Court is ironic.

Editor Thomas L. Haskell presents the goal of the book as affording "the alert reader an opportunity to situate the 'crisis of the professions' in a far deeper context than is ordinarily recognized" (p. xx). If the collection seems to be somewhat dispersed in meeting that goal, it may well be because of a lack of interaction. Three of the essays were presented at a symposium sponsored by the Minnesota Humanities Commission in observance of the centenary of the graduation of Thorstein Veblen from Carleton College. Along with the two mentioned above, they include a fine presentation by Dorothy Ross, "American Social Science and the Idea of Progress." Nevertheless, these and the other

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six articles do not by themselves tie the questions together. Some sort of response or reply to the central issues would have been clarifying and perhaps would have advanced the dialogue beyond the disparate collection presented here. The book comprises three sections: "Defining the Problem," in which Freidson's essay is joined by a Marxist analysis of professions by Magali Sarfatti Larson; "Science and the University," consisting of four articles; and "Professional Communities," which deals with the communities of practitioners.

That two of the articles were previously published and a third is part of a book in progress in part explains the disjointed nature of this presentation. Three other essays were funded, at least in part, by the National Endowment for the Humanities or the National Science Foundation, which leads to the intriguing question of why the book does not treat the influence of granting agencies and their institutional biases on the autonomy of the professions. That would have been a useful addition to the section on science and the university, in particular. Another lacuna is the absence of any cross-cultural or international treatment. If this collection dealt only with the histories of the professions in America, this absence would be understandable, but, for a book dealing with theory, a more comparative approach would have added a great deal. A comparative approach might have thrown into greater relief such issues as the degree to which the professional value of autonomy in America reflects an American cultural value, perhaps in contrast to the legal profession in a single-party system or the medical profession in a socialist economy.

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ALEJANDRO MALASPINA. *Viaje científico y político a la América Meridional, a las Costas del Mar Pacífico y a las Islas Marianas y Filipinas verificado en los años de 1789, 90, 91, 92, 93 y 94 a bordo de las corbetas Descubierta y Atrevida de la Marina Real, mandadas por los capitanes de navío D. Alejandro Malaspina y D. José F. Bustamante*. Madrid: Museo Universal. 1984. Pp. viii, 740.

In this volume the diary and related documents of Alejandro Malaspina from his five-year expedition to the Americas and the Pacific Basin from 1789 to 1794 are reproduced from a Madrid publication of 1885. The original work, prepared and edited by Pedro Novo y Colson, used Malaspina's detailed account of the lengthy voyage, which, while being annotated for publication, was consigned to obscurity because of court intrigue and political confusion. The volume of 1885 was Spain's first attempt to give Malaspina, a fitting competitor to British Captain James Cook and the French Count La Pérouse, his proper place in history. The new version has resulted from Spain's rekindled interest in the efforts and accomplishments of its illustrious scientist-navigators of the late eighteenth century.

The present editors, Juan Barceló, Mercedes Palau, Aránzazu Zabala, and Blanca Sáez, have used the same manuscript as Novo y Colson (MS. 753 in Madrid's Naval Museum) but have eliminated repetitive material and the descriptions of the separate travels of Malaspina's co-commander, José de Bustamante. They have added a new introduction, expanded the footnotes substantially, and included some two hundred pages of appendixes. The first appendix contains twenty-six letters written by Malaspina from 1790 to 1798 to his friend Count Paolo Greppi and to his brother Giancinto. These, and an introduction by Emmanuele Greppi, give insights into the life of Malaspina and help explain his fall from favor. In his reports about the Americas, Malaspina suggested some sweeping governmental changes and criticized the policies of Manuel Godoy. He was arrested on November 24, 1795, tried, convicted, and held in prison for eight years in La Coruña. After a period of exile in his native Italy, Malaspina was allowed to return to Spain but was unable to secure publication of any materials. He died in 1809.

The five-hundred-page diary of the expedition covers Malaspina's preparations for departure, the personnel involved, and an account of each place visited. Their first stop at Montevideo in September 1789 was followed by brief visits to Argentina, the Malvinas (Falklands), Chile, Peru, Ecuador, Panama, Costa Rica, Nicaragua, and then Acapulco in March 1791. They left a scientific detachment in Mexico and continued to Alaska, Nootka Sound, and California. Following their return to Acapulco, the expedition visited the Marianas, the Philippines, Australia, New Zealand, and Tonga, recrossed the Pacific to Peru, and returned to Spain via Chile and Argentina. The ships arrived in Cádiz in September 1794. Accompanying the diary are about seventy original illustrations of ports, buildings, persons, fauna, and flora. These considerably augment the few copper plate engravings included in the 1885 edition.

The second appendix contains biographical sketches of all persons connected with the expedition and the third devotes seventy pages to the identification of toponyms. The volume includes a glossary of nautical terminology. These sections vary in quality but are valuable additions. The bibliography contains the current literature pertaining to Malaspina's voyage.

This laborious effort brings to the public an extremely useful reference work on a voyage that in many ways outshined the efforts of Cook and La

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Pérouse. The reset edition, although large in size, is clearly printed and easy to use. It contains much that is new but lacks a critical introduction discussing recent work on Spain's role in the late eighteenth-century European world. It also suffers from a number of typographical errors in the English-language portions. Despite these minor problems, it is an important point of departure for future efforts concerning Malaspina in particular and Spanish naval exploration in general.

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ANCIENT

DONALD B. REDFORD. *Akhenaten: The Heretic King*. Princeton: Princeton University Press. 1984. Pp. xxvi, 255.

Academic controversy and modern myth surround Egypt's reforming king Akhenaten (ca. 1353–1336 B.C.), who proclaimed a new solar religion, closed temples, and persecuted the state god Amun, only to have his work undone and memory reviled. Evaluations of the man range from James Henry Breasted's "first individual in history" to deranged egomaniac. Donald B. Redford presents an important new picture of Akhenaten, the first synthesis since Cyril Aldred's *Akhenaten* (1968), of which a completely new edition is about to appear.

Redford encloses Akhenaten's reign within a sketch of the political history of the Eighteenth Dynasty (ca. 1540–1290 B.C.), focusing on his predecessor Amenhotpe III. He presents at length his own "Akhenaten Temple Project," which is reconstructing the decoration of the king's early Karnak temples and excavating the site of one of them (pp. 63–136). For many readers, this may be the best part, because the material is new, discussed absorbingly and in detail. Scenes show the rapid development of the king's ideas and document ceremonies and beliefs unattested or superseded during his central years. They also demonstrate that Queen Nefertiti's prominence, greater than that of any other Egyptian queen, started early (her eventual fate—she disappears from view before Akhenaten's death—is uncertain).

The book's innovatively conventional image of Akhenaten's politics is a great advance on earlier discussions, incorporating much recent material. Akhenaten did not have a twelve-year coregency with Amenhotpe III. Performing a conventional Egyptian role, he mounted a campaign to the far south and attempted to maintain his "empire" in the Near East (here one misses the insights of Mario Liverani). A problem Redford ignores is the identity of Akhenaten's ephemeral successor, whom Rolf Krauss and others have shown to have been a woman. Here, the book's semipopular character is awkward. Parts are quite thickly referenced, but much is unsupported. When his interpretation is one among several, one would be happier if he indicated the existence of alternatives.

Redford soberly discusses the central religious questions of the reign, but his conclusion reverses this, saying that Akhenaten's worship of his god, lacking mythology, was "in the truest sense of the word, atheism" (p. 234). This is an outsider's view of a deity with pronounced personal qualities of rulership and care for his world (how would it apply, for instance, to Islam?). Redford translates a fragmentary new text with a discussion of Akhenaten's and the other gods, in which the other gods are said to "cease" and their "bodies," which he has studied, are denigrated. Nothing can be like the sun and its rays (p. 172). In relation to traditional Egyptian beliefs, this concerns as much the complex manifestations of deities as their natures. Jan Assmann has shown that Akhenaten's demythologizing was foreshadowed under Amenhotpe III without a break with earlier iconography, and Peter Munro has published an example of his god's characteristic dogmatic name that may predate his reign. Thus, Akhenaten's thought built on what went before, although it was more rigorous and intolerant and was revolutionary in its visual presentation. Akhenaten was not Redford's "heretic" so much as the ruler who, by attempting to impose orthodoxy, invented heresy.

Redford's book, slanted toward political history and his own research, will be valuable for many years. Space remains for a more comprehensive work on Akhenaten, and this book often uses unexamined criteria of evaluation.

JOHN BAINES
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P. J. CASEY. *Understanding Ancient Coins: An Introduction for Archaeologists and Historians*. Norman: University of Oklahoma Press. 1986. Pp. 160. $22.50.

P. J. Casey has accomplished a major task: a precise and succinct analysis of the current multiple course of study in ancient Greek and Roman numismatics. No other volume on the subject matches its intent and content. Although often omitted in the curricula of ancient history, classics, and art history, numismatics remains, nevertheless, a highly significant auxiliary academic field by which to illuminate and clarify ancient and medieval social and economic conditions and significant political transformations. Strongly centered on Rome, the focus of Casey's own research but also the area most fruitful for his socioeconomic analyses, this monograph offers the

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advanced student and the scholar more than a sound introduction. The emphasis on numismatic methodology is rewarding and presents to the beginning student sufficient tools by which to enter numismatic studies. In-depth studies of coin hoards, coins as archaeological objects *in situ*, and their patterns of distribution enable students to elucidate political frontiers, spheres of trade and influence, and weight and metallic composition that indicate movement from one political and economic sphere to another. Casey's clear descriptions communicate his profound understanding and elucidate the extensive data presented.

In museums the coin ranks among the minor arts and as a single object has little historical merit beyond its mint of origin and metrological weight. Yet the broad study of a coin within a hoard, or among others from an excavation, can reveal to the numismatist historical patterns omitted in the ancient literary texts or even patterns contradictory to those texts. Thus, numismatics becomes an ancillary yet primary means of study for the ancient historian, especially for the Hellenistic and Roman imperial periods. The economic aspects of coinage predominate: patterns of deposit can be investigated, social trends uncovered, and the evidence of inflation identified.

The great strength of Casey's monograph is his study of the processes rather than of objects, the means by which students of ancient societies can learn how to analyze hoards and coins from archaeological sites and apply to historical studies the recently developed scientific analyses of those coins, such as neutron activation analysis. Casey's work is less description than a sound introduction to new historical techniques.

JACK MARTIN BALCER
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PIERRE VIDAL-NAQUET. *The Black Hunter: Forms of Thought and Forms of Society in the Greek World*. Translated by ANDREW SZEGEDY-MASZAK. Foreword by BERNARD KNOX. Baltimore: Johns Hopkins University Press. 1986. Pp. xxiv, 367. $28.50.

Together with Jean-Pierre Vernant and Marcel Detienne, Pierre Vidal-Naquet has long been an admired interpreter of Greek representations of self, society, and the cosmos. This translation, marking an important stage in the dissemination of his work to English speakers, is limited to Vidal-Naquet's structuralist analyses. Its emphases and vision of Greek society are well illustrated by the titles of its four parts: "Space and Time," "The Young, the Warriors," "Women, Slaves, and Artisans," and "The City, Vision, and Reality." The book comprises fifteen essays, including several highly original pieces: the title selection "The Black Hunter and the Origins of the Athenian *Ephebeia*," "Land and Sacrifice in the *Odyssey*: A Study of Religious and Mythical Meanings," "Slavery and the Rule of Women in Tradition, Myth, and Utopia," "Athens and Atlantis: Structure and Meaning of a Platonic Myth," and "A Study in Ambiguity: Artisans in the Platonic City" (somewhat less impressive). Although not quite on the same level, the following critical collections of evidence are useful: "The Tradition of the Athenian Hoplite," "Recipes for Greek Adolescence," "Reflections on Greek Historical Writing about Slavery," and "The Immortal Slave Women of Athena Ilias."

"The Black Hunter" has pride of place, since, for the author, it "not only . . . occupies a central position in the economy of the work as a whole but also because writing that piece marked a significant advance for me: the discovery of structural analysis as a heuristic device" (p. xix). This far-ranging essay explores the cults, mythology, and instruction within the Athenian *Ephebeia*, the two years of military training before youths became citizens. Marginalization characterizes the *Ephebeia* in that ephebes behaved in a manner opposite to adults. In contrast to the heavily armed hoplites, who guarded the city's fields, the light-armed ephebes ranged the borderlands, the area of hunting rather than agriculture, as guileful tricksters, not as open, direct combatants. Here, Vidal-Naquet explores fundamental polarities such as that between males and females (expressed in ritual transvestism); in "Land and Sacrifice" he examines, not coincidently, another set of polarities. The socioreligious contexts within the *Odyssey* exemplify human or political space. Social people cultivate the earth for subsistence; wild food does not come to them without labor, and they are not cannibals. People in the space of the polis cannot commune with the gods without rituals such as sacrifice. Beyond this world lie both the subhuman realm of the cyclopes and the "utopia" of the Phaeacians, exempted from the customary instrumentality for fulfilling wants. Attitudes toward social existence are revealed by Odysseus's crossing spatial boundaries, by nearing or retreating from his home.

Vidal-Naquet is less successful in analyzing applications of institutions (especially economic) to situations, particularly where their use was an outgrowth of policy. His treatment of slavery in "Reflections" and "Were Greek Slaves a Class?" fails to examine the continual compromises (often capitulations) made by servile ideologies to economic realities. Another weakness is an over-generalizing from the consistency of terminology to imply a static underlying social order, while the data are viewed synchronically. Problems become noteworthy in an

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embarrassing piece, "Epaminondas the Pythagorean, or the Tactical Problem of Right and Left," which establishes the Theban's decision to lead with his left wing and refuse battle with his right as a result of his liberation from the symbolic code establishing the primacy of the right wing. Vidal-Naquet ignores the demographic, tactical, and political reasons why Epaminondas's actions had been impractical earlier. He considers Thucydides' explanation for the right-handedness of battle lines in their right-handed combatants a mechanistic interpretation or a "rationalization"!

Andrew Szegedy-Maszak has translated ten essays and has included five previously published translations, which are readable renditions. Bernard Knox provides a useful survey in his foreword (an abbreviation of a review of the French edition of 1981; the original is better consulted [*New York Review of Books*, March 3, 1983]).

I anticipate that historians will find the publication of this volume opportune for familiarizing themselves with Vidal-Naquet's structuralist explorations, which are not as well known as they deserve to be (and are to philologists).

THOMAS J. FIGUEIRA
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MALCOLM ERRINGTON. *Geschichte Makedoniens: Von den Anfängen bis zum Untergang des Königreiches*. Munich: C. H. Beck. 1986. Pp. 294. DM 38.

Two themes have dominated the writing of ancient Macedonian history: biographies of famous kings (such as Philip II, Philip V, and Alexander the Great) and accounts of the Macedonian-Roman encounters that led to Roman domination of the Balkans. The paucity of evidence, especially for the period before Philip II, has severely hampered attempts to produce synthetic narrative histories of the kingdom of Macedon. This historiographical deficiency is being remedied by the continuing multivolume publication of N. G. L. Hammond *et al.*, *A History of Macedonia* by Oxford, but the utility of this work is offset by its forbidding format and the fact that the initial volumes reached print too early to take into account the archaeological discoveries in northern Greece since 1977.

The author of a new work is Malcolm Errington, one of the leading scholars of Macedonian politics. His major contribution has been to challenge the traditional view that the inner workings of Macedon rested on a sophisticated *Staatsrecht*. In a series of papers—apparently preliminary studies for this book—Errington argued that the political and military organization of Macedon rested largely on the authority of the monarchy and the degree to which it could command the loyalties of Macedonian barons. This model is not unknown to students of less ancient European history.

Politics, diplomacy, and military affairs dominate this well-constructed account, which outlines the course of affairs from Macedon's archaic origins down to its submersion into the Roman dominion in the mid-second century B.C. Anthropology, geography, and archaeology are of little interest to Errington; his attention is centered on the monarchy and how it exercised influence both within and without the variable frontiers of the kingdom. The book is about equally divided between a narrative account of the Argead dynasty to its end ca. 310 B.C., and an examination of Hellenistic rule of the Antigonids.

Of special value is a section that treats the organization of the Macedonian state as a separate theme, and here the author sets out the thesis that characterized his early work: at the center of the state was the king. The absolutism of the Macedonian king reached fruition under Philip II—although one can observe precedents for the exercise of royal authority among some of the monarchs of the fifth century B.C.—and continued to provide the pattern for monarchical control among the Antigonids.

Errington emphasizes the ways in which Macedon was different from the world of the Greek city-states, an outlook that will undoubtedly discomfit those who have argued that the Macedonians were just another tribe of Greeks. Intended perhaps as an advanced textbook for German-speaking universities, this work will appeal to scholars and students alike as a clear exposition of modern thought about the Macedonian kingdom. And long after the advance of modern archaeology inevitably invites a reconsideration of some sections of the book, its description of the internal organization of the Macedonian state will remain succinct and sensible.

EUGENE N. BORZA
Pennsylvania State University

GEORGE BRAUER, JR. *Taras: Its History and Coinage*. New Rochelle, N.Y.: Aristide D. Caratzas. 1986. Pp. ix, 231. $55.00.

George Brauer, Jr., has written a survey of Taras (Tarentum) from its semimythical beginnings down to the last recorded event in its history, when an irate brothel keeper sold an unwilling prostitute in its market place (from the *Ephesian Tale* by Xenophon of Ephesus). One may suspect that Brauer came to the subject out of an interest in the varied and sometimes magnificent coinage of the Tarentine mint, which is abundantly illustrated and carefully discussed in the text. His work can be recommended to beginning students as clearly expressed, thorough in treatment, and well buttressed

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in the sources and modern literature; factual bobbles are few.

Moses Finley, however, expressed in his last book (*Ancient History: Evidence and Models* [1985], p. 61) his dismay at "a spate of pseudo-histories of ancient cities and regions in which every statement or calculation to be found in an ancient text, every artefact finds a place." To support his criticism, Finley dissected P. M. Fraser's *Ptolemaic Alexandria* (1972) as a lengthy example of "anachronistic antiquarianism," but he could well have used the present work. In the opening chapters Brauer often has to present "a tissue of conjecture" (p. 45), and our understanding of the character, significance, and causes of decline of the Greek colonies in south Italy is not very markedly advanced in this account, which does not support any real hypotheses. There are a fair number of typographical errors; the most intriguing results in the appearance on several pages (pp. 54ff.) of "the agnostic horseman" on some Tarentine coins (read "agonistic").

CHESTER G. STARR
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RICHARD A. BAUMAN. *Lawyers in Roman Transitional Politics: A Study of the Roman Jurists in Their Political Setting in the Late Republic and Triumvirate.* (Münchener Beiträge zur Papyrusforschung und antiken Rechtsgeschichte, number 79.) Munich: C. H. Beck. 1985. Pp. xiv, 148. DM 60.

This is the second in Richard A. Bauman's proposed three-volume study of the role "lawyers" played in Roman republican and imperial politics. The volume, like its predecessor, is an admirable collection of ancient references to lawyers and legal issues with an excellent presentation of secondary material.

Two themes are at work in this slim book, not a third the size of the first. One is the transition of legal experts from politically prominent individuals (senators) to others who, employing the law as a means of social, economic, and political mobility, used their expertise to enrich themselves and to win promotion to office or equestrian status. The later lawyers also frequently served the needs of the *imperiosi* of the late Republic. This second theme is the most important, and the most controversial, contribution of the study. Both lawyers and legal expertise were in transition during the late Republic, and the examination of the period shows that both were headed toward imperial patronage and service.

The volume is primarily devoted to an examination of one key figure, Servius Sulpicius Rufus (consul, 51 B.C.), his students, and his contemporaries. His relationship with Cicero illustrates the first theme: Cicero believed, in opposition to Servius, that oratory and military service were more advantageous to political advancement than was legal knowledge. Servius's professional service in the cause of Pompey and Caesar and his role as peacemaker in the immediate aftermath of Caesar's assassination are the major concerns of the volume. Servius was also harbinger of the empire since he counseled Caesar on Octavian's adoption (adrogation) and was instrumental in obtaining the first privileges accorded that young man, who owed everything to a name. The most interesting feature of the study is the author's attempt to make various issues turn on legal and constitutional questions, which lawyers such as Servius had the legal expertise to address. A harsher critic might say they legitimized questionable practices by their skill in supplying legal precedents and justifications. The author is not overly concerned with this problem. More's the pity.

RICHARD E. MITCHELL
University of Illinois

E. P. SANDERS. *Jesus and Judaism.* Philadelphia: Fortress. 1985. Pp. xiv, 444.

As the title suggests, this volume is a continuation of E. P. Sanders's earlier studies on Paul and Rabbinic Judaism. The structure of this work is as follows: an extended opening section in which the author offers a historical reconstruction of Jesus and his relationship to Judaism; analyses of the themes of the restoration of Israel, the Kingdom, conflict, and death; a conclusion that traces the links between Jesus' aims and achievements and Jewish restoration eschatology. The strategy of the book is to begin with evidence that everyone can agree on and then to argue on this basis to the meaning of three kinds of factors: the facts about Jesus, the outcome of what he said and did, and the place of these words and deeds in first-century Judaism.

As Sanders sees it, Jesus expected the restoration of Israel and thus favored keeping the law. That he had this attitude toward the law is confirmed, we read, by his disciples' observation of the law after his death. Sanders sees as valid the observations of J. Klausner and G. Vermes, who, according to Sanders, "do not find any substantial points of disagreement between Jesus and his contemporaries, and certainly not any that would lead to his death" (p. 55). Jesus' undoing with the authorities stemmed from his challenge to the temple, which he announced would be destroyed and rebuilt. His fellow Jews shared these views, but Jesus stated them in such a provocative way and accompanied them by such unauthorized actions as to elicit hostility from the priestly leaders. He did not cleanse the temple

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for the benefit of the gentiles, as the Gospels report, and he uttered no call to national repentance, since his message was addressed mainly to individuals (p. 117).

Complicating his relations to the authorities was the attraction of crowds to his healings and exorcisms. Indeed, this was the sole significance of working miracles, since Jewish tradition attached no special importance to them, in spite of some references to miracles in the later prophets. Sanders doubts the authenticity of the familiar "finger of God" passage, which assigns special meaning to the exorcisms, and sides with Morton Smith's assessment of these acts as basically attention-getting devices.

Jesus did not envision that gentiles would be admitted to the Kingdom. In fact, he did not have any developed plan or practical strategy as to how the Kingdom would come (pp. 226, 235). His claim to speak for God was not unique, although his inclusion of sinners was distinctive. He was not a reformer (p. 269), and no evidence suggests that he opposed or rejected parts of the law. Rather, he joined with other Jews in regarding obedience to the whole law as an essential requirement for the people of the covenant in the future restoration. The gospel accounts in which Jesus challenges the purity and sabbath laws are not historical. By choosing twelve followers, by his claim to speak for God, and especially by his actions in the temple, he offended his contemporaries. The priestly leaders arranged to have him put to death, although the accounts of his trial are wholly unreliable. Paul, and others in his name, expanded on the challenges he raised and made the radical break of including the gentiles. This movement grew in ways that would have been unforeseeable in the time of Jesus.

How are we to assess this reconstruction? Early in this work, Sanders attacks Albert Schweitzer's theory about Jesus as the messenger of thorough-going eschatology on the grounds that Schweitzer's view did not arise from the texts but was imposed on them and that nothing within Judaism of the first century corresponded to this notion of Jesus' message. The reader may respond to Sanders's theories with, "Physician, heal thyself!" Sanders's earlier work has rightly noted that determining the grounds of inclusion in the people of the covenant was the paramount issue in Judaism in the post-Maccabean period. Yet, in evaluating the New Testament evidence about Jesus, Sanders implicitly assumes that the question had one basic answer—the one that later took definitive form in the Mishnah and Talmud. He feels free to draw on later evidence, as his glib remark, "give or take 200 years," shows (p. 276). He has no patience with the attempt to examine the details of the Jewish evidence in terms of the larger world view that lies behind and gives shape to it (p. 128), as represented by Jacob Neusner's efforts to show the changing outlooks within Judaism in this period.

What Sanders sets forth as "facts," therefore, are a mixture of widely accepted assertions (for example, that the Romans executed Jesus as a royal pretender) and arbitrary conclusions. The latter function in thoroughly procrustean fashion and simply dismiss the evidence that lies in the texts. Although one might agree that the intensity of the hostility toward the Pharisees that appears in Matthew 23 reflects a later situation (in my opinion, a conflict during the Jamnia developments), Sanders's reconstruction is arbitrary. It results in a picture of Jesus as a bumbling gadfly, orthodox in intent but misguided in tactics, whose followers converted his movement into something that bears little resemblance to his intentions.

The reader of Sanders's book is urged to follow it with *Rebecca's Children* (1986) by Alan Segal, who sees Jesus as revolutionary in his recasting of the hopes of Israel. In my opinion the evidence from the Gospels and from first-century Judaism confirms Segal's reconstruction, not Sanders's.

HOWARD CLARK KEE
Boston University

MEDIEVAL

ELIZABETH SIBERRY. *Criticism of Crusading, 1095–1274*. New York: Clarendon Press of Oxford University Press. 1985. Pp. xii, 257. $34.95.

Ernest Baker remarked in his formative article in the eleventh edition of the *Encyclopaedia Brittanica* (1911) that the Crusades were the "'foreign policy' of the papacy, directing its faithful subjects to the great war of Christianity against the infidel" (vol. 7, p. 524). In view of the constant and predominant role of the papacy in Western medieval history and of its close patronage of the Crusade and the crusading ideal for the better part of four hundred years, the paucity of modern works on criticism of the Crusades is surprising. Elizabeth Siberry sets out to reverse the hypothesis of Palmer A. Throop (*Criticism of the Crusade* [1940]) that crusading motivation declined in the thirteenth century and that this decline largely resulted from public antagonism. Her contrary conclusions are that, despite "bitter resentment of abuses, fundamental criticism of the concept itself was rare; and in 1274 [Second Council of Lyons] the expeditions in Europe and the Latin East still enjoyed considerable support from the faithful" (p. 220).

Siberry's book is well researched and comprehensively documented, with extensive notes and a volu-

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minous bibliography of primary and secondary works. Her sources reflect her interest in the popular response to crusading and consequently are more literary than legal. She divides her study into two parts: first, criticism of the crusaders and, second, criticism of Crusades.

The title is somewhat misleading for little in the book deals with criticism of the Crusade per se. The author seeks to show that the church's crusading policy was widely and generally supported in the twelfth and thirteenth centuries, hence "criticism," as discussed in part 1 particulary, was directed toward the relative inability of crusaders to live up to popular expectations of their mandate. Chapter 1, "The Qualifications of a Crusader," might better have been titled "Unsuitable Crusaders," whereas chapter 2, "Commitment to Crusading," is primarily a list of admonishments to those who delayed their departure on the Third and Fifth Crusades, followed by the conclusion that public pressure finally persuaded procrastinating kings to fulfill their vows on the third. In part 2, chapter 4, a valuable study of the financing of the Crusades, the author argues that royal and papal taxation were met with increasing resistance. This criticism, too, Siberry argues, was directed not toward the institution of the Crusade but toward taxation and "the impoverishment of the church" (p. 143). The chronological focus of the book is on the thirteenth century, and the geographical focus lies in France and, especially, England.

Siberry's research is extensive and useful. Confronted by the wealth of critical material she has gathered, however, some controversy may arise over her repeated insistence and general conclusion that negative criticism of the Crusade and of papal policy toward it was personally rather than popularly motivated and that such criticism can be explained by the geographical location and the particular circumstances from which it arose.

MICHAEL GERVERS
University of Toronto

ERIK WAHLGREN. *The Vikings and America.* (Ancient Peoples and Places, number 102.) New York: Thames and Hudson. 1986. Pp. 192. $22.50.

Erik Wahlgren has spent his leisure since retirement writing this little volume that will be placed on the coffee table to join the ever-growing pile of picture books on the Vikings. Intended for a general audience, the book covers the same territory as Magnus Magnusson's, *The Viking Expansion Westwards* (1973). More than half of the text and illustrations is used to treat the culture of the Vikings and their westward expansion before the brief settlement in North America. To this material the author adds synopses of his own two previous works, a book in 1958 on the Kensington stone and a long article in 1969 on the two sagas dealing with the discovery of the New World, a work prompted by Helge Ingstad's spectacular but, by now, well-known archaeological findings at Lance-aux-Meadows. Scholars generally agree that the landing site in Newfoundland cannot be the Vinland mentioned in the sagas. Speculating on a possible location in a final chapter, Wahlgren takes up a suggestion first made by Erik Reman in the 1940s that Leif the Lucky landed at Passamaquoddy Bay in southern New Brunswick, on the border between Canada and Maine.

The author's real interest is American Norsemania, the gullible acceptance of any hoax of alleged Norse origin. Demonstrating the dangers that occasionally follow a little learning, the longest chapter, "Buckram Viking," gives a full list of such frauds, kept up to date since the book on the Kensington stone, to which the author devotes his considerable linguistic talents of deciphering. Although such manifestations may interest students of American-Scandinavian culture, they are not the concern of prolonged, serious scholarship of Scandinavian medievalists.

The book is written with what the dust jacket calls "verve and wit," perhaps suitable for the intended audience, but historians may find the style cute and trite when—to take just one example—they are reminded that the medieval Greenlanders neither had their minds sedated with television nor were bothered by the chattering wireless (p. 87). The book has excellent maps.

JENNY M. JOCHENS
Towson State University

BRIGITTE KASTEN. *Adalhard von Corbie: Die Biographie eines karolingischen Politikers und Klostervorstehers.* (Studia Humaniora, Düsseldorfer Studien zu Mittelalter und Renaissance, number 3.) Düsseldorf: Droste. 1986. Pp. 227. DM 68.

Recent years have seen a spate of monographs on Carolingian political and ecclesiastical leaders such as Paschasius Radbertus, both Hincmars, Agobard of Lyons, Theodulf of Orleans, John Scottus, Alcuin, Ratramnus of Corbie, and Adalhard of Corbie's half-brother, Wala. Brigitte Kasten's contribution to our understanding of the leading spiritual and political personalities of the first Europe is a worthy contribution to the literature.

Adalhard has not been a neglected figure. Three studies preceded that of Kasten—four, if one counts the *vita* Paschasius Radbertus wrote in 826 shortly after Adalhard's death. Each of Kasten's modern predecessors followed Paschasius in portraying the abbot as a "second Antony," after the example of the

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third-century Egyptian monk. Adalhard certainly was a monk, but Corbie was not the desert. Corbie's position in the Carolingian world and Adalhard's family connections insured that he would play a leading role not only in early ninth-century monastic life but also in the maelstrom of Carolingian politics. The subtitle of Kasten's book announces her intent to underscore Adalhard's political activity. The abbot's long life spanned the period from 751–52 to 826, from when Pepin was mayor of the palace to when his grandson Louis was emperor. Adalhard moved from the cloister at Corbie to Italy, where he served as regent for two Carolingian kings. Political intrigue and dynastic strife led to Adalhard's subsequent banishment for seven years. His traditional, practical brand of monasticism ran afoul of the reforming movement headed by Benedict of Aniane. Adalhard finished his years overseeing the foundation of two new monastic houses, Corvey and Herford.

Adalhard's writings touched on a number of interesting topics, and these are all explored dutifully with a firm grip on the sources and the most recent scholarly literature. As Kasten admits, her forays into genealogical questions yielded essentially negative conclusions. More frustrating was her inability to write true biography. She discusses Adalhard's "personality" in a few pages late in the book and concludes that he was a traditionalist and a gifted man with many interests. Kasten's study is worth reading for the light it sheds on Carolingian politics and monastic life.

JOHN J. CONTRENI
Purdue University

ROY MARTIN HAINES. *Archbishop John Stratford: Political Revolutionary and Champion of the Liberties of the English Church, ca. 1275/80–1348*. (Studies and Texts, number 76.) Toronto: Pontifical Institute of Mediaeval Studies. 1986. Pp. xviii, 587. $45.00.

Roy Martin Haines's work with the episcopal registers of the medieval diocese of Worcester has resulted in regular, rich harvests for students of English church history. His monograph describing the administration of the diocese in the first half of the fourteenth century (1965) inspired a biography (1978) and a calendar of the Worcester register (1979) of Adam de Orleton, politician and bishop who served the sees of Hereford, Worcester, and Winchester. Haines's newest work is an exhaustively researched study of the career of Orleton's colleague in royal and ecclesiastical service, John Stratford.

Stratford and Orleton lived through a momentous period in English history and, as royal councilors and ambassadors, exercised some influence on a course of events that determined the destiny of their homeland. Stratford's name derived from Stratford-on-Avon, where he was born into a wealthy, middle-class family in the 1270s or 1280s. Educated in civil law at Oxford, he found employment as a legal advisor to the cathedral chapter at Worcester and went on to build a brilliant career in royal service rewarded by ecclesiastical preferments. He attained the see of Winchester during the last years of the reign of Edward II. He was active in the events that led to Edward's abdication, and he served as chancellor in the government of his son, Edward III. The young king's military ambitions in Scotland and France precipitated the Hundred Years' War and strained the English economy. When fiscal problems frustrated royal policy, Edward turned against Stratford and accused him of malfeasance. In conscious imitation of his famous predecessor at Canterbury, Thomas Becket, Stratford undertook to protect the church against encroachment by the state. These events were played out in the context of Parliament, for the development of which these were crucial years.

Haines's mammoth study (431 pages of text and notes and an additional 140 pages of appendixes, bibliography, and index) is a model of careful research. Probably, no significant reference to Stratford in the contemporary documents was overlooked or unrecorded. Interpretations are cautious, and, in a concluding section, Haines points out how the prejudices and preconceptions of earlier historians led them beyond the evidence in their attempts to explain Stratford's career. Haines is inclined to give Stratford the benefit of the doubt and cautions that he should be judged by the standards of his own day—not a later generation's preference for holy men who eschew worldly ambition. The weakness of the book lies in its often repeated claim to be a biography. The sources do not contain enough detail to permit the writing of biography in the usual sense. They show that Stratford took part in meetings, participated in ambassadorial missions, and sat in Parliament. But they rarely reveal his individual contributions to the deliberations of these bodies. At the end one is left with a clearer picture of Edward III (about whom far less is said) than of Stratford. Biography may be a genre destined perpetually to frustrate medievalists.

A. DANIEL FRANKFORTER
Pennsylvania State University

MODERN EUROPE

JOAN KELLY. *Women, History, and Theory: The Essays of Joan Kelly*. (Women in Culture and Society.) Chi-

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cago: University of Chicago Press. 1984. Pp. xxvi, 163. $18.00.

Over a decade ago Joan Kelly first asked, "Did women have a Renaissance?" With this simply stated and obvious question, Kelly opened up a vast new field in women's history and transformed the ways in which historians look at the history of Western societies. Such a significant contribution can be attributed to few scholars.

In this collection of essays we see Kelly working out her ideas at the theoretical and applied levels. This gives the collection a liveliness and a historic value that transcend both the individual essays and the answers they offer. To read the essays is to take a journey through the evolution of feminist thought and historical methods in the last two decades.

The earliest, though not the first, essay in the collection is "Did Women Have a Renaissance?" Written in the early 1970s, the essay's importance lies above all in its title. For, regardless of how one views women's position in the Renaissance, the title effectively challenges, from the perspective of gender, traditional periodization. Having posed the question for the Renaissance, Kelly went on to ask it of all historical epochs. In "The Social Relation of the Sexes: Methodological Implications of Women's History" (1976), she wrote that the result of this examination yielded "a fairly regular pattern of relative loss of status for women precisely in those periods of so-called progressive change" (p. 2). Conventional history is thus turned upside down. Classical Greece, Renaissance Italy, the France of the French revolution—all the great ages of progress in liberty, knowledge, and cultural achievement for man—are ages that brought about a loss of freedom for woman.

A great deal has been written in support of and in opposition to this rewriting of Western history, and the final outcome of the debate is far from clear. But the possibility that women's history has points of difference with that of men has been formulated and cannot be ignored.

Yet Kelly was not eager to create a separate sphere for women's history or even to advocate that traditional periodization was in all respects inadequate. She tried to create a relational system of periodization, so that the activities, power, and cultural evaluation of women were assessed by comparison and contrast with those of men. Her purpose was to discover the institutional and social mechanisms that linked the two sexes and resulted in divergent outcomes for both. "Handled in this way," she argued, traditional periodizing concepts may well be retained . . but in the evaluation of such changes we need to consider their effects upon women as distinct from men" (p. 4). This meant that women should not be studied in isolation, just as the poor should not be studied without reference to the rich.

Because Kelly was a Marxist as well as a feminist, she wanted to reconcile class with sex as a social category and as a tool for historical analysis. Although the tools of class analysis might be applied with useful results to women's lives, she argued, along with Gerda Lerner, that women are not a class, a caste, or a minority. Rather, they are a distinct social category, "a social opposite, not of a class, a caste, or of a majority, since we are a majority, but of a sex: men" (p. 6).

Kelly was brilliant in her elaboration of how class and the division of labor affected the status of women. In the "Doubled Vision of Feminist Theory" (1979), she showed how socioeconomic and gender systems simultaneously operated to give men and women different social status. She thus brought together feminist and Marxist concepts and considerably modified both. The oppression of women, she claimed, could no longer be dismissed, as many Marxists had done, as a vestigial remnant of the capitalist system. At the same time, radical feminists could not argue that the relation between the sexes was inherently antagonistic and unvarying in the suppression of women regardless of race, class, or society. A unified view, Kelly believed, enables us to understand how societies reproduce their existing socioeconomic and gender systems so that patriarchy has persisted in feudal, capitalist, and socialist societies.

An unresolved problem with Kelley's analysis is how to incorporate it into theories of social change. Kelly wanted to develop better notions of how economic innovations affect gender roles as well as how changes in family life influence socioeconomic arrangements. Yet she encountered difficulties in proposing adequate explanations. Despite the importance of "Did Women Have a Renaissance?" for a new kind of history, for example, it lacks a convincing explanation of why changes took place in the position of Renaissance women and of how economic transformations influenced women of different classes. The essay claims that the developments that opened up new possibilities for Renaissance men, namely the consolidation of the state and the emergence of capitalism, affected women adversely. Yet, why these transformations had a negative effect on women is not stated. If the argument is that women were increasingly relegated to the domestic sphere, which cut them off from a growing market economy, then one can point out that most economic production, whether done by male or female labor, took place in the home until the advent of the factory system. This, of course, would have a bearing on working-class women but would be irrelevant for aristocratic women who faced different kinds of economic pressures.

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Kelly's arguments are also flawed with respect to the evidence. Her essay rests on a comparison of literary sources from medieval France with those of Renaissance Italy. This is surprising given her Marxist perspective. Other historians of women have gained a closer look at the lives of women, and especially working-class women, by using archival documentation.

The research agenda of feminist scholars in the next few years will include the search for new evidence and new conceptual tools to help solve these problems. If Kelly had lived longer, she might well have been a leader in these endeavors. Kelly considered feminist theory a "conceptual vision," a perspective. Like Leon Battista Alberti, whose notions of perspective were the subject of her first study, she was open to new ways of looking at the world because she knew that it looked different when viewed from a variety of angles. This openness led her in the fruitful directions that others will follow and undoubtedly modify. Her attitude was the antithesis of those who attempted to vilify her, calling her a "saint" followed by religious zealots. (See G. R. Elton, "History According to Saint Joan," *The American Scholar* 54 [1985]: 549–55.) Ridicule has always been the tool of the intellectually bankrupt. In contrast, Kelly offers insightful historical scholarship based on the application of a rigorous intellect to difficult questions. Clio is much enriched by her work.

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PAUL H. D. KAPLAN. *The Rise of the Black Magus in Western Art.* (Studies in the Fine Arts; Iconography, number 9.) Ann Arbor, Mich.: UMI Research. 1985. Pp. xi, 319.

This book is a classic example of a certain kind of iconographical study. Circumscribed in its subject, it seems to record nearly all the antetypes and appearances in literature and art of the Black Magus, one of the three exotic kings present at the Adoration of the Christ child. The text painstakingly catalogues the evolution of the black male in medieval and early Renaissance art from his first appearance as a royal attendant to his triumph as an imperial type in some well-known northern Renaissance paintings and engravings. (A few references are also made to female images.) Along the pathway to the devastating outcome of cross-cultural contact we find some welcome surprises. When they first entered Western medieval art, blacks were seldom portrayed pejoratively. Instead, they were emblematic of all distant and noble saviors who set themselves against the Muslim infidel; the mythical Prester John is an especially striking example.

Paul H. D. Kaplan does not simply offer a visual chronology but details "first" and "lost" references in literary and historical accounts and enumerates lines of influence from fables and travelers' accounts across nearly eight hundred years. All of this detail is most impressive. Based on fifteen years of research, the book's abundance of documentation is staggering—so monumental, in fact, that it gets in the way of a critical reading of the material.

The list of information assembled here pretends to grow out of no theoretical framework. Yet what has been identified as "objective" history has long been recognized as being a political agenda in itself. Not interpreting is, of course, an interpretive strategy. By implicitly asserting that its subject can be dealt with by reciting facts and recording them in chronological order, the study undermines recognition that the image of blacks in Western culture was politically determined. At only a few moments does the author engage the political aspect of the configuration of this mythology, for example: "The proliferation and standardization of the black Magus/King may have been a convenient means of disguising and deflecting consideration of the moral and theological dilemmas posed by black slavery" (p. 105). In general, the book offers no socioeconomic analyses beyond a few obligatory references to this developing slave trade.

The historical contextualization, apart from some references to the ecumenical propaganda of the Holy Roman emperors (the image was predominantly a German product), is also thin. For example, although the author admits that the image of the Black Magus grew in popularity after 1347 (p. 85), he makes no connections with the social, religious, and political upheavals caused by the ravages of the Black Death. One would also expect to find some acquaintance with anthropological analyses, such as studies of schemes of classification of the "other," but such potentially corroborating ideas are notably absent.

Still, the book provides a useful service. Kaplan has gathered a vast amount of documentary evidence that will surely be employed by another scholar to more critical ends. There is more than one way to attend to details and elaborate the archive. We cannot complain about the author's assiduousness in examining the records, but to ignore contexts—to assume, as this work does, that its scholarly goal can be achieved only by extirpating its subject from its contexts—is in this case particularly disconcerting.

MICHAEL ANN HOLLY
University of Rochester

JOYCELYNE G. RUSSELL. *Peacemaking in the Renaissance.* Philadelphia: University of Pennsylvania Press. 1986. Pp. x, 278. $34.95.

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Reading this intelligent study, I felt above all a sense of awe and admiration for the indefatigable nineteenth-century archivists and editors who made it possible. Except for one manuscript source, every document cited is in print. Joycelyne G. Russell's study, however, proves more than a traditional diplomatic history: in her nicely conceived first chapter, she introduces the religious, philosophic, historical, and chivalric traditions known to the rulers and statesmen of the sixteenth century and considers how these traditions shaped their thinking and behavior in regard to making peace. She finds pertinent statements from the fathers of the church, medieval scholastics, and Renaissance humanists, all full of biblical and classical references, and effectively contrasts them with the bellicose language of popular chronicles and chivalric romances. In the course of her study, she does not neglect the subject of peace and war in the fine arts and public spectacles.

This particular dimension Russell gives to peacemaking in the sixteenth century seems the book's chief contribution to scholarship. Her treatment of the traditions of the papacy and leading states is useful, but her remarks on procedure, apart from providing fresh examples, breaks no ground not turned by Garrett Mattingly and Gaston Zeller. The fine chapter on the peace conferences at Calais and Bruges in 1521 promoted by Henry VIII and Cardinal Wolsey is based on an article she published in 1974 and fits well the argument of this book.

Nearly one hundred pages of text are given to a detailed study of the negotiations and personalities involved in the Peace of Cateau Cambrésis of 1559, which has not to my knowledge received such thorough treatment before in English. England did not fare well in the treaty, yet Elizabeth, by playing off her ally Philip II of Spain and her enemy Henry II of France, father-in-law of Mary Queeen of Scots, secured her throne and was free to revive the Reformation. Russell only mentions this and other larger issues, since her focus is the conduct of peace negotiations. In an epilogue she suggests certain long-term developments she finds rooted at Cateau Cambrésis and puts in proper perspective some of the Protestant mythology about the treaty's aims. From a remark by Jean Bodin in 1576 she unfortunately concludes, "Papal arbitration had become a thing of the past" (p. 225). Pope Clement VIII Aldobrandini could hardly have thought so when he worked for the Peace of Vervins of 1598.

Her bibliography of primary sources in print and secondary works is impressive, and her footnotes attest that she used much more. That most are in English and French is not surprising. Many key Spanish documents were lost to shipwreck when Philip II returned from Brussels to Spain, and much of what survived was removed to Paris under Napoleon and remained there until 1942. Thus, French scholars made the most use of them. Also, much Spanish material appears in English in the *Calendar of State Papers*, Spanish series, thanks to the labor of M. A. S. Hume and others in Spanish archives. Given Russell's aims, I do not believe more Spanish references would have made much difference in this very thorough study.

PETER PIERSON
Santa Clara University

GUSTAV HENNINGSEN and JOHN TEDESCHI, editors. *The Inquisition in Early Modern Europe: Studies on Sources and Methods*. Assisted by CHARLES AMIEL. Dekalb: Northern Illinois University Press. 1986. Pp. x, 241. $27.50.

This de facto *Festschrift* in Marcel Bataillon's memory is an excellent collection of papers on research in progress. Most of the essays are derived from the Copenhagen conference on the five-hundredth anniversary of the Spanish Inquisition. Gustav Henningsen's and John Tedeschi's survey is an important piece in its own right; three articles on Spain, four on Italy, one on Portugal, and one on the medieval inquisition follow. Each essay is characterized by lucidity, persuasiveness, and thorough knowledge of primary and secondary materials. The volume is an indispensable historiographical and bibliographical tool as well.

The editors suggest that these essays mark the emergence of a new methodology, "archivology," through which we can gain new insights into early modern European social history, especially that of Iberia and Italy. Archivology is the recording and describing of manuscript collections, reconstructing the original archives' arrangements, describing bureaucratic "literary" genres, and identifying channels of command for the transmission and reception of information. The alert reader will be struck by some obvious parallels with modern society; indeed the relevant subtitle of the significant article by Jaime Contreras and Hennigsen is "Analysis of a Historical Data Bank." In more ways than one, as suggested imaginatively here, the Suprema was a primitive computer, if one substitutes the innumerable human cogs working for it and its branches for modern machines.

If archivology is the method, then the inspiration comes from, among others, Mikhail Bakhtin, Fernand Braudel, Emmanuel LeRoy Ladourie, Carlo Ginzburg (a contributor), and Natalie Zemon Davis: these statistical records are avenues to realizing their orientations and emphases. The fields of inquisitorial inquiry and actions that have traditionally attracted scholars' attention are not overlooked or downplayed, however. But, following the editors'

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opening manifesto, the contributors show that, beginning in the mid-sixteenth century, the various *inquisitions participated in a campaign to reform* their peoples' morals and bring them into greater conformity with Tridentine guidelines of faith and dogma. Thus, the inquisitions increasingly attended to charges less spectacular than crypto-Judaism, Protestantism, or Mohammedanism. And, of course, the great killing of heretics occurred in the earlier, dramatic period, despite periodic flare-ups as late as the Spanish Judaizing trials in the early eighteenth century. Several articles note, too, the nearly inevitable slide into routine by the early to mid-seventeenth century, which affected even social and religious controls. The essays clearly portray national and regional differences as well as similarities; space does not permit the detailed examination they deserve. Also, the traditional view that the inquisition was an arm of the state gets some support here, much as some recent work on Valencia has shown.

To a considerable degree the inquisition was effective in Iberia; a chapter heading in Henry Kamen's *The Inquisition and Society in Spain* (1985), "Silence is Imposed," is a fitting epitaph. Seemingly long ago now John Elliott's eloquent conclusion to his *Imperial Spain* (1963) foreshadowed many of the results of the Holy Office's work shown here. From other perspectives scholars such as Mary Elizabeth Perry and William Christian point to areas reflecting some of the attitudes of the ecclesiastical and, often, secular authorities described in these pages. This admirable collection both refines and expands our knowledge of the Mediterranean inquisitions and marks out areas for old hands and newcomers to research.

PAUL J. HAUBEN
University of the Pacific

UMBERTO MAZZONE and ANGELO TURCHINI, editors. *Le visite pastorali: Analisi di una fonte.* (Annali dell'Istituto Storico Italo-Germanico, Quaderni, number 18.) Bologna: Il Mulino. 1985. Pp. 270. L. 20,000.

This volume represents the first fruits of a move to create a center, named after Hubert Jedin, for the study of the post-Tridentine church within the Institute for Italo-Germanic Historical Studies at Trent. Gatherings of Italian, French, and German scholars have led to an ambitious, long, and complex project to organize and assist research into the institutional history of the Roman Catholic Church in Western Europe chiefly, though by no means exclusively, during the centuries following the end of the Council of Trent in 1563.

The decrees of Trent began to give the episcopal visitation of the diocese, which in the later Middle Ages had been practiced fitfully but sometimes with vigor and care, the form and character of a regular, comprehensive oversight of the spiritual, social, and economic life of the church as a whole. This form came to be exemplified, in Italy for instance, by the work of prelates such as the Borromei. Besides visitations carried out by bishops themselves, other pastoral visitations were conducted by archdeacons, archpriests, and their equivalents, records of which survive. The registers of such visitations have come down to us in bewildering numbers, often catalogued poorly or not at all, unevenly distributed across time and region, and compiled according to criteria that evolved subtly through the centuries. The richness and confusion of such data alternately entice and daunt the historian: Angelo Turchini tells the story of how, as a necessary preliminary to his historical work, he had from 1969 onwards to inventory the pastoral visitation records and later the entire diocesan archive of Rimini (p. 139).

Editors Umberto Mazzone and Turchini and the contributors have therefore attempted to describe some procedures, share experiences and methods, and make available as much information as possible to assist and harmonize research into the records of pastoral visits, with the hope of ensuring that the results of different research projects will emerge as mutually compatible data for the historian intent on a synthesis. To this end the book consists of three sections that cast some preliminary light on the question. First, three essays discuss the "state of the art" in the analysis of visitation records in France (Marc Venard), Germany (Peter Thaddäus Lang), and Italy (Turchini). Second is an enumeration of some model "questionnaires" proposed for French and German registers respectively and a plan for a similar scheme for Italian records. Such questionnaires involve a complex exercise in creating schematic "calendars" of the material surviving for a given visit, bishop, diocese, or period; they are indispensable if the records are to be made manageable. Third, and finally, there is a list of 608 theses on topics relating to pastoral visitations written by Italian research students from 1958 to 1984.

Evidently, this volume is a technical manual, especially relevant to the needs of the Italian researcher and mostly preoccupied with rarified problems of organizing and classifying historical materials. Between the lines, however, one gathers some interesting insights. Venard, especially, makes some stimulating points about the way the French visitation registers reflect the changing priorities of their respective ages: limited first by late medieval particularism and then by conflict with the Reformation, such visits grew to a peak of detail and effort

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toward 1700 and declined (somewhat) into routine in the eighteenth century.

A further, perhaps irreverent, thought is provoked by this symposium. In spite of the best efforts toward internationalism, national characteristics peep through: Venard's essay is an elegant exercise in *longue durée* history, Lang's a cautious, rigorous survey of one aspect of the question (contemporary "questionnaires" from the sixteenth and seventeenth centuries), and Turchini's is a highly abstract, sometimes wordy, brilliant preliminary discussion. With such diversity in approaches, one feels that the degree of "harmonizing" will be limited. Perhaps this is just as well: the vision of platoons of researchers marching in well-drilled lines through the ecclesiastical archives of Western Europe is less appealing than the present state of fruitful confusion tempered by the proper regard for usefulness and comparability that this book seeks to inculcate.

EUAN CAMERON
University of Newcastle upon Tyne

CALIXTE HUDEMANN-SIMON. *La noblesse luxembourgeoise au XVIII^e siècle*. Foreword by JEAN MEYER. (Publications de la Sorbonne, Histoire moderne, number 18. Section historique de l'Institut Grand-Ducal, number 100.) Paris: La Sorbonne or Section Historique de l'Institut Grand-Ducal, Luxembourg. 1985. Pp. ix, 616. 225 fr.

Serious social history of the French nobility began only in 1960, with the appearance of Robert Forster's *Nobility of Toulouse*. In the generation since, historians have produced a series of case studies of both families and regional communities, extending their researches across France and into Francophone areas that lay outside the French state of the Old Regime. As a result, the condition of the nobility has become central to debate about the causes and meanings of the French revolution.

Calixte Hudemann-Simon's fine study both adds a further case study to this literature and poses an essentially new question for students of the nobility. Novelty is imposed by the geographical context that she has chosen: within the large Luxembourg of the eighteenth century (three and one-half times as large as today's), German and French feudalisms met. Comparison between them is a central theme of the book and produces some of its most striking findings. German nobles within the duchy, Hudemann-Simon shows, relied more heavily on seigneurial dues and made stronger claims for aristocratic privilege than did the French. Even at a point of direct contact with French society and institutions, German feudalism remained sharply different and imposed a much heavier burden on society.

This comparison aside, Hudemann-Simon's research focuses on the central problem of the nobles' successes and failures in adapting to eighteenth-century social and political change. Were the nobles becoming wealthier or poorer over the eighteenth century? More or less numerous? How did they respond to the modernizing efforts of the state, in this instance the Habsburg state as led by Maria Theresa and Joseph II? How did the Enlightenment touch them?

Hudemann-Simon's answers would once have seemed conventional, for she finds a nobility declining in nearly every respect. Concerned to limit family size to limit divisions of their estates, the nobles failed even to reproduce themselves over the eighteenth century, and relatively few ennoblements replaced families that died out or moved from the area. The Luxembourgeois followed egalitarian inheritance divisions, which meant the reduction of family circumstances with each generation. Elsewhere, nobles might compensate for such losses by holding government office or by profiting from the economic currents of the eighteenth century, but there were few such hopes in Luxembourg. The centers of political power were distant, and the region was poor; seigneurial revenues counted for nearly half the revenue of a typical estate. Nobles held a predominant place in the region's principal industry, iron founding, but the industry decayed as the century advanced.

Above all, the nobles of Luxembourg responded with unwavering hostility to political and intellectual innovation. They disliked the Enlightenment and remained committed to racialist explanations of their social position and of the inferiority of other groups. They found the bureaucratic state an infuriating enemy, and it regarded them with contempt. All of this is far from the pictures of aristocratic dynamism, adaptation, and success that William Doyle and Guy Chaussinand-Nogaret have proposed.

This is a finely crafted work, based on a large number and broad range of documents, and employing careful, sensible modes of analysis. Only the book's concluding section, on culture and mentalities, seems less richly documented and more limited in method. The exposition proceeds carefully and slowly, in the manner customary among *thèses de troisième cycle*, and much in it will mainly interest specialists. Hudemann-Simon's conclusions deserve wide attention, however, for they speak directly to current debate about the nobles' role in eighteenth-century society.

JONATHAN DEWALD
University of California, Irvine

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JOHN ROBERT MCNEILL. *Atlantic Empires of France and Spain: Louisbourg and Havana, 1700–1763*. Chapel Hill: University of North Carolina Press. 1985. Pp. xvii, 329. $32.00.

In a fresh and engaging study based largely on research in archives and libraries in Spain, France, Great Britain, and the United States, John Robert McNeill compares French and Spanish colonial policies and their consequences by examining the defensive outposts and entrepôts of Louisbourg and Havana from the early eighteenth century to their respective captures by the British during the Seven Years' War.

McNeill examines Havana and Louisbourg within a broad imperial and international setting before considering the physical, social, and economic characteristics of each. The ports' military roles, he stresses, were limited to local defense because the Spanish and French crowns were unwilling or unable to fund a navy capable of challenging the British. The Spanish crown was more concerned than the French with maintaining tight economic control over its colonies and more effective in securing the desired results, despite the drain of contraband. Mercantilist policies worked during peacetime, and profits from Louisbourg and Havana flowed to France and Spain. British naval superiority during times of war, however, underscored the limited degree to which the Bourbons could benefit from their Atlantic empires and bridge the gap between theory and reality in their imperial systems.

McNeill convincingly shows that the real value of Havana and Louisbourg lay in providing Spain and France with tangible economic benefits. The French expected Louisbourg to serve as an entrepôt for trade between Quebec and France and between Quebec and the French West Indies as well as a major fishing port. The inability of Quebec to provide adequate foodstuffs for Louisbourg, however, led to increased trade with New England. As the cod fishery declined in the late 1730s, commerce increased in importance. By 1749 the value of Louisbourg's trade with New England exceeded that of its trade with France. Commerce had replaced the export of cod as the port's major activity.

McNeill challenges conventional wisdom that the British capture of Havana in 1762 was the turning point in Cuba's history. A royal tobacco monopoly that processed and sold almost exclusively Cuban tobacco was already providing the crown with an annual profit of over ten million pesos. Royal policies to increase sugar production and reduce Spain's reliance on imports from outside the empire had been successful as well. An extensive trade in contraband, in which Cubans exported tobacco and hides and imported slaves and manufactured goods, also played an important role in the Cuban economy. Before 1762 the legal trade, exports, and slave population were more extensive than previous authors have stated, and, thus, the Spanish colony was already poised for the emergence of a sugar monoculture.

This study is a fine example of comparative history. McNeill's mastery of archival sources makes the work a contribution to the histories of Louisbourg and Havana as well as to imperial history. Historians of colonial Spanish America will be particularly grateful for his elucidation of royal policy and Cuban economic and social realities from 1700 to 1763, a period peculiarly neglected in colonial historiography.

MARK A. BURKHOLDER
University of Missouri, St. Louis

ALAN PALMER. *Crowned Cousins: The Anglo-German Royal Connection*. London: Weidenfeld and Nicolson; distributed by Biblio Distribution Center, Totowa, N.J. 1985. Pp. x, 255. $22.95.

In recent years the writing of books on the British royal family has become a cottage industry that has kept many a professional author in reasonable comfort and an academic scribbler in beer money. A trip to the local shopping mall bookstore will reveal a stack of works on royal families, marriages, babies, children, sailors, palaces, and even funerals, with the modern "royals" taking pride of place. "Charles and Dianas" have become nearly as ubiquitous as "Harlequin Romances." The reason for this deluge, which—as Marie Antoinette observed—always seems to come *apres* the monarchy, are obvious. Books on kings, queens, princes, and princesses find a ready publisher and even readier buyers.

The criteria for assessing them for a scholarly journal do not readily come to mind. Popularity need not be a handicap, neither should style—unless of course the journal is in sociology. Instead, those neglected historical virtues, such as depth of research, saying something new, quality of judgment, and the ability to communicate to the educated layperson, should be our standards.

In such a light Alan Palmer can be credited with doing a good job in this book. He starts by noting that every reigning royal family and claimant in Europe (except for the Albanian) are descended from the offspring of Frederick, the Elector Palatine and James I's daughter, Elizabeth. Since their marriage on St. Valentine's Day 1612, the links between Britain and Germany have been strong, particularly during the reigns of the first two Georges, with the Hanovarian connection, and during that of Queen Victoria, with her marriage to Albert and her

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daughter's to Frederick II. In this century of two world wars with Germany it is hard to remember that many English agreed with Queen Victoria that Germany "*ought* to be our *real* ally" and that after her funeral *Punch* thanked the kaiser, "God Bless you, Sir! England will never forget your genuine Sympathy!" The outbreak of World War I thirteen years later ended the royal closeness between England and Germany, something which the Duke of Windsor failed to revive when he flirted with Hitler.

Sometimes Palmer's judgments, such as his attempts to whitewash Edward VIII (p. 222) and his suggestion that Elizabeth II "rules" rather than reigns (p. 226), are suspect. He tends to dwell on the minutiae of the complicated and tedious relationships between the various English and German princely houses in the nineteenth century and falls into Queen Victoria's mistake of too closely identifying them with those of their respective national states. But altogether this is an enjoyable book that academic historians as well as educated laypeople could profitably pack for a weekend trip to the beach.

CHARLES CARLTON
North Carolina State University

MICHAEL I. HANDEL, editor. *Clausewitz and Modern Strategy*. Totowa, N.J.: Frank Cass. 1986. Pp. 324. Cloth $30.00, paper $14.95.

Since the appearance of the new translation of Carl von Clausewitz's *On War* (1976) by Michael Howard and Peter Paret, interest in the teachings of the great Prussian soldier-philosopher has revived. At the same time, the Vietnam debacle has forced American military theorists to modify their expectations about the efficacy of technology and to rethink their long-standing preoccupation with the methodical and prescriptive ideas of Henri de Jomini. For the last ten years military historians and analysts have given considerable attention to Clausewitz's concepts, especially his insistence on the centrality of politics in war and the ever-present uncertainties of the battlefield.

This collection of essays, first presented during a conference at the U.S. Army War College in April 1985 and initially printed in *The Journal of Strategic Studies*, can be taken as further evidence of this revival. The majority of the contributors are, or formerly were, affiliated with the college, the Naval War College, or other postgraduate service schools. The others, Jehuda Wallach, Martin Van Creveld, Werner Hahlweg, and Franz-Jürgen Müller, are well-known Israeli or West German military historians.

The fourteen essays (Jay Luvaas has contributed two) are ably edited by Michael I. Handel and address various aspects of Clausewitz's work, centering on his famous *On War* but also using some of his minor studies. The essays are grouped into three parts. The first, largely introductory, asks whether writings over a century and a half old still are relevant in an era when war has been revolutionized by technology. Van Creveld maintains that the principles remain eternal, whereas Handel suggests that technological change demands some modification of Clausewitz's framework.

The second part deals with a number of specific themes and topics discussed by Clausewitz, including the problems of chance and uncertainty, time and space, and the role of intelligence as they are treated in *On War*. Although competently done, these pieces are too narrowly conceived and exegetic to be impressive. An essay by Hahlweg, claiming that Clausewitz made a major contribution to the analysis and theory of guerrilla war also is unconvincing. The exception here is Luvaas's contribution on the influence of Frederician and Napoleonic warfare on Clausewitz, which, although providing no new interpretation, difficult in any case when there have been so many, certainly is a splendid introduction.

Finally, the third section takes up Clausewitz's legacy and denies the belief that soon after its publication *On War* became the bible of the Prussian army, the foundation of its victories, and an inspiration for Western military theorists. As Luvaas points out, both Basil Liddell Hart and J. F. C. Fuller found Clausewitz incoherent and confused his style with substance. For that matter, both Wallach and Müller find that this was also true in Germany, where strategic thinkers tried to derive operational lessons from Clausewitz while reversing his doctrine concerning the primacy of politics. William Murray agrees but argues that even so the German military system managed to incorporate Clausewitz's thinking in training and approaches to combat. Finally, Douglas Porch and John Gooch look at his impact on military thought in France and Italy. In a vigorous essay Porch asserts that the French selectively adopted some elements of Clausewitz, above all his faith in the superiority of the moral over material factors, whereas Gooch shows that the Italians remained ignorant of his work and did not develop any unified theory of war.

Almost all the essays note that soldiers everywhere tried to draw operational lessons from Clausewitz but rejected his contention that political objectives and not abstract strategic goals were paramount. Despite these common themes, the collection remains somewhat fragmentary, and few readers will be able to sustain interest through the entire vol-

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ume, but then this perhaps is the inevitable fate of published conference papers.

GUNTHER E. ROTHENBERG
Purdue University

WILLIAM M. CALDER III and DAVID A. TRAILL, editors. *Myth, Scandal, and History: The Heinrich Schliemann Controversy and a First Edition of the Mycenaean Diary.* Detroit: Wayne State University Press. 1986. Pp. 273. $35.00.

Heinrich Schliemann is one of a select group—the great pioneer archaeologists of the nineteenth century who laid the foundations of a new discipline while revealing the existence of cultures hitherto unknown. His career and personality have fascinated many and irritated others. Only recently, however, has the debate over Schliemann become vitriolic. Anger pervades almost every page of this book, edited by William M. Calder III and David A. Traill. The editors' thesis is simple. Schliemann was a bad man. He "was ill, like an alcoholic, a child molester, or a dope-fiend. He did not know the difference between right and wrong" (Calder, p. 37). Therefore, he must be reckoned a bad scholar and archaeologist. "A man scrupulously honest with his income tax is apt to be honest elsewhere, while an adulterer will cheat in other ways as well" (Calder, p. 29). We must conclude that he "deliberately distorted the archaeological record" (Traill, p. 130). In a series of essays and in the critical notes attached to an edition of Schliemann's Mycenaean diary, Traill and Calder seek to discredit Schliemann both as a man and a scholar. A further essay by W. Schindler is also hostile. Only fourteen pages of the volume are given to the case for the defense in an essay by H. Döhl, "Schliemann the Archaeologist."

There is room, and even need, for a careful re-examination of Schliemann's life and findings. What emerges here, however, draws the reader's attention as much to the motives of Traill and Calder as to those of Schliemann. Time and again, insinuation takes the place of proof. "I suspect that on occasion Ernst Meyer, without telling us, may even have added passages to some of Schliemann's letters" (Calder, p. 22). The notes to the edited diary are replete with innuendo. "Though one might suspect that Kolobos is a figment of Schliemann's romantic imagination, letters from Kolobos exist" (n. 126 pp. 212–13). "One might be tempted to infer from this that Sophia's supervision of the excavation of the treasury was no more real than her presence at the discovery of 'Priam's treasure.' There is a letter, however" (n. 163 p. 217). Traill finds a "tone of disappointment" (n. 202 p. 221) in Schliemann's diary entry for November 15, "In the tomb of the 2 unsculptured tombstones were found only the calcinated remains of 2 more bodies, *each exactly with* as many ornaments . . . as the first body." Traill argues, "Given the remarkable wealth of these discoveries the tone of disappointment is surprising and almost suggests that Schliemann *knew* that much more impressive finds were still to be made." Schliemann's accuracy is called to account over the reporting of gold buttons (Traill, n. 185 p. 219), but the inaccuracy is that of Traill, who has failed to notice that Schliemann distinguished between total number of buttons and total number of gold buttons. Traill's caution in an earlier essay about Schliemann's intentions in having duplicates made becomes certainty in the introduction to the diary. "It is now clear that in 1873 Schliemann seriously considered having duplicates made of some of the gold and silver pieces in 'Priam's treasure' with the intention of passing them off as authentic finds" (p. 133).

The case is not compelling. Moreover, it is ahistorical. In their relentless and ultimately unproductive pursuit of imagined textual inaccuracies, Calder and Traill show no understanding of the historical context in which nineteenth-century archaeology developed. In quite another context C. Brink has objected to "an attempt to psychologize an assumed failure rather than explaining an historic achievement" (*English Classical Scholarship* [1986], p. 165). As Brink maintains in the case of A. E. Housman so can we say for Schliemann: "It is only [the Victorian context] together with the critic's reaction to it, that enables us to make historical sense of his scholarly achievement" (Brink, p. 167). Schliemann's career must be set against nineteenth-century scholarship and opinion.

Schliemann's diaries are essential tools in such an endeavor and, thus, Traill's clearly edited form of the diary is valuable. The Mycenaean diary reveals the development of the infant discipline of archaeology and of a self-educated archaeologist. The weaknesses of both are plainly visible. But there is more than weakness. Schliemann sought to analyze as well as preserve the material he excavated; he revised after re-examining; he re-created the mood of the work. To learn that Schliemann twice misuses the word "cleave" believing that "it could mean 'split' in the intransitive as well as the transitive sense" is not of great moment (n. 195 p. 220; n. 261 p. 227). A proper understanding of Schliemann's role in the field of archaeology demands more than innuendo and textual criticism.

C. G. THOMAS
University of Washington

DONALD J. OLSEN. *The City as a Work of Art: London, Paris, Vienna.* New Haven: Yale University Press. 1986. Pp. xiii, 341. $35.00.

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This book, a synthesis of social, cultural, and environmental history, presents an important argument that compels the attention of urban historians. The specialist will learn much, but the subject matter is not specialized. Donald J. Olsen claims that nineteenth-century Paris, London, and Vienna "were deliberate artistic creations intended not merely to give pleasure but to contain ideas, inculcate values, and serve as tangible expressions of systems of thought and morality" (p. 4). Throughout the book, he emphasizes the differences between Vienna, Paris, and London: planning, housing design, social geography, and suburban development are separately described for each city, with many comparisons and cross-references. He shows that people carefully balanced uses of all the public and private, interior and exterior aspects of a city, such that the study of the city as a whole becomes a prerequisite to an understanding of its diverse parts.

The wealth of examples drawn from the study of nineteenth-century periodicals and books, both specialized publications and travel literature, combined with Olsen's marvelous insights into the social uses of style and space and his splendid choice of 142 photographs and illustrations make this a most attractive and comprehensive survey of upper-class urban life. In a book so dependent on contemporary literary and graphic descriptions, however, a discussion of urban commentary in nineteenth-century culture and of conventional modes of perception would have been appropriate. The absence of sustained discussions of the political context of building and of the structure and economy of architectural practice and the construction industry are easier to overlook. These omissions do not, I believe, compromise his overall conclusion: that visual delight was one of the most significant purposes of urban design.

Olsen accepts beauty as a worthy attribute of city building; his greatest accomplishment in this book may be to argue that urban aesthetics should be treated by historians as something as important as, for example, patterns of social mobility, the distribution of wealth, and the politics of municipal reform. Scholarly treatments of environmental design are few and, for the most part, emphasize a biographical or institutional approach to architecture and planning, leaving aesthetic issues to scholars with a background in the design fields. The city, Olsen says, is itself a document, and he has made a considerable effort to examine the evidence, in what remains of nineteenth-century Paris, London, and Vienna, for himself. But, then, he knows how to look. Olsen is at his best when making his own observations rather than summarizing and analyzing secondary works. Perhaps the last chapters are the most provocative. Here, he interprets different styles in terms of political and social ideologies and explains the historicism of architectural design in the nineteenth century as something progressive and creative. Olsen believes that a sense of history helped city builders in the nineteenth century create a more beautiful and more livable urban environment (p. 310); in this book he has made a compelling case for urban historians to broaden their own vision of the past.

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GORDON MARTEL, editor. *The Origins of the Second World War Reconsidered: The A. J. P. Taylor Debate after Twenty-Five Years*. Boston: Allen and Unwin. 1986. Pp. 276. Cloth $29.95, paper $11.95.

A. J. P. Taylor, in his celebrated book on the origins of World War II, was guilty of a multitude of historical sins—if we are to accept the judgments of the specialists assembled by Gordon Martel. Taylor was illogical, inconsistent, contradictory, simplistic, confused, perverse, careless, superficial, naive. He indulged in "fatal cleverness," clichés, paradox, nonsense, and "artful obfuscation." He was given to knocking down straw men, viewing history as "a seamless web of aphorisms" (p. 50), and writing on both sides of most questions. His "pellucid prose" often concealed "opaque meaning." He missed the fundamental point of Adolf Hitler's foreign policy and failed to take any of Hitler's ideas seriously; he disliked and misunderstood both the Poles and the French; he based his judgment of Benito Mussolini on "a bizarre misconception" (p. 88); he constantly gave Russia the benefit of any doubt. Taylor's "heroes" were Hitler and Neville Chamberlain (p. 246), but men such as Chamberlain and Kurt von Schuschnigg were responsible for the war (p. 247).

No doubt the foregoing paragraph is an unfair pastiche of Martel's collection of essays—although every one of those epithets and accusations appears somewhere therein. One might be led to wonder, after such a hostile barrage, why anybody could possibly take Taylor's book seriously—why its publication stirred up such a storm, why it has remained on assigned reading lists in countless university classes, and why, after twenty-five years, it should inspire reconsideration by a dozen talented Canadian and American historians (most of them of the post-Taylor generation). Obviously, the work of this hardened and impenitent sinner must have some redeeming social value.

To be sure, most of the authors admit (though often grudgingly) to considerable admiration and respect for what editor Martel himself calls "a historical classic" (p. 32). They speak of its compulsive readability and of the sense of excitement it produces in lethargic undergraduates. They recognize

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that Taylor's book antedated the opening of rich new archival sources that allow more accurate judgments, and they grant that some of his anticipations proved to be shrewd guesses. More important still, some of them believe that the impact of *Origins* on the historical profession has been in some ways constructive. As Martel puts it, "He rescued this vital part of the human story from the vapid simplicities of good versus evil and returned it to its proper place of complexity and paradox" (p. 13). And, in Norman Rich's words, "It has compelled every student of the Nazi era to re-examine his own views about the subject, and over the years some of the ideas once considered controversial have become part of the conventional wisdom" (p. 119).

Taylor has expressed some alarm at the idea that *Origins* may have become the new orthodoxy, even though his declared goal in writing the book was "to tell the story as it may appear to some future historian" (*Origins*, p. 16). Presumably, he thought that the passage of time would erode the passions and prejudices evoked by World War II and permit more impartial judgments. For those of us who lived through the events that he describes, Taylor's provocative assertions may indeed have had a healthy effect by forcing us to look again at some of our easy assumptions. One wonders, though, if that virtue will hold for a new generation that lacks direct experience of Hitler and of Nazi ideology in action. That generation, exposed to *Origins* without such direct experience to counteract it, may be inclined to swallow Taylor's version whole. If so, the cure that he has sought to provide may prove worse than the disease it was to remedy. Sometimes in history the "vapid simplicities of good and evil" are not so vapid after all. Surely, one value of Martel's collection is that it may serve as a cautionary tale for those inclined to fall under the spell of Taylor's persuasive prose and clever paradoxes.

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MARY PRIOR, editor. *Women in English Society, 1500–1800*. New York: Methuen. 1985. Pp. xvi, 294. Cloth $33.00, paper $9.95.

Although the inclusive dates are 1500–1800, the essays in this collection deal mainly with the sixteenth and seventeenth centuries. In her foreword Joan Thirsk elaborates on the development of women's history and contrasts the present essays based on the analyses of small groups (widows in Abingdon, tradeswomen in Oxford, wet nurses in Somerset and Hertfordshire, diarists in the seventeenth century) with the work of earlier British women's historians such as Agnes Strickland and Elizabeth Strickland, Ivy Pinchbeck, and Alice Clark.

Dorothy McLaren's essay, "Marital Fertility and Lactation," is one of the most interesting and reinforces but also neglects previous work, clarifying the relationship between breast-feeding of infants and long intervals between births. Wealthier mothers put their infants out to nurse and gave birth to large numbers of children, "always going to bed, always pregnant, always giving birth" (p. 46). Poor mothers sold their milk by breast-feeding the children of the rich as well as their own and thus controlled their fertility by prolonged and nonexclusive lactation. One would like to know who made the decision to put the babies out to nurse, and for what reasons. Was it the fathers, who wanted sexual access to their wives and found this way to break the taboo against intercourse during breast-feeding? Alas, McLaren's sources of parish birth records do not offer this information.

Sarah Heller Mendelson analyzes and lists twenty-three intriguing diaries of Stuart women of the gentry and aristocracy. As one would expect among this group, "gender was apparently more important than class in shaping the basic pattern of their lives" (p. 191), which, unlike those of men, were divided into three distinct stages: virginity, marriage, and widowhood. Although a number of diarists feared to abandon the freedom of their virgin state, the majority of marriages in these diaries are described as "loving and companionable" (p. 193). Spirituality and introspection pervade the mood of these documents, which deal in passing with domestic, social, and political details.

In her study on remarrying widows Barbara J. Todd concludes that, as the early modern period progressed, widows were (for varying reasons) less likely to remarry. The essays on Tudor bishops' wives (Mary Prior) and Tudor and Stuart recusant women (Marie B. Rowlands) offer interesting comparisons and contrasts. Both groups of women were expected to remain firmly within the private sphere. Queen Elizabeth's pronounced dislike of married clergy particularly restricted the wives of her bishops, for they were not able to appear publicly or deport themselves like aristocratic ladies who might be considered of equal social or political rank. Catholic women, especially in times of persecution, achieved great spiritual and administrative heights. Among the recusants, the intrepid Mary Ward's continental houses and schools for "English ladies" demonstrate the opprobrium suffered by women who ventured into the public sphere during this period.

Patricia Crawford's useful "Provisional Checklist of Women's Published Writings, 1600–1700" includes a short introduction and statistical analysis that highlights the phenomenally sharp rise in

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women's publications at and immediately after the time of the civil wars. Crawford states that the writers' own words suggest that they felt obliged to publish, despite public and familial hostility to female authors, because they appreciated that their experiences were so different from those of men. In so doing, these seventeenth-century authors have given the most important reason for the value of "women's history."

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NESTA EVANS. *The East Anglian Linen Industry: Rural Industry and Local Economy, 1500–1850.* (Pasold Studies in Textile History, number 5.) Brookfield, Vt.: Gower, 1985. Pp. 178. $35.50.

The best justification for this book is the remark of a colleague, an extremely knowledgeable historian, on seeing it in my possession: "I didn't know East Anglia had a linen industry." It has long lived in the shadow of the better-known East Anglian woolen industry, whereas historians of linen have concentrated on the Northwest of England, Scotland, and Ulster. Within Suffolk and Norfolk itself the manufacture of linen—using hemp rather than flax—was a subsidiary occupation to farming and other forms of textiles. Nevertheless, in the later seventeenth and eighteenth century linen production was of considerable importance to small cultivators in the wood-pasture districts, where the growing of hemp—better known to modern readers as cannabis—and the production of coarse linen provided a valuable income.

Nesta Evans is to be congratulated on a meticulous piece of detective work in rediscovering the industry. Since its output satisfied a local demand for cheap cloths and its main raw material was locally produced, the industry has left few traces in the major sources available to historians of the early modern period. Business records also do not exist, save for a few scraps from the later nineteenth century, when the industry was almost dead. So the author has been forced to rely on probate inventories and wills for the period before the 1730s and newspapers and travelers' reports thereafter. The slimness of the volume is a scant reflection of the enormous labor involved in writing history from such fragmentary sources, although the many footnotes thanking other local historians for information testify to the vitality of English regional history.

The story is of an industry that existed in the later Middle Ages and Tudor period, but which grew in the seventeenth and early eighteenth centuries before entering a long decline that ended in virtual extinction by 1850. Generally, the author is better at describing than explaining, although cheap rural labor and locally grown hemp were clearly the raison d'être for the industry's growth. The story could easily be fitted into modern notions of "proto-industrialization," but Evans is not partial to jargon. She might have explained the decline of the industry more convincingly had she been better informed about the history of Ulster linen production (her remarks on page 122 indicate she has not read the standard history by Conrad Gill published in 1925). Still, she tells an interesting story and has written a genuinely worthwhile piece of history.

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CHRISTOPHER COLEMAN and DAVID STARKEY, editors. *Revolution Reassessed: Revisions in the History of Tudor Government and Administration.* New York: Clarendon Press of Oxford University Press. 1986. Pp. viii, 219. Cloth $37.00, paper $13.95.

Despite an opening paragraph that applauds Geoffrey Elton for his "brilliant and imaginative interpretation of the crowded events of the 1530s" (p. 1), the coeditors of this reassessment of *The Tudor Revolution in Government* have come to bury Elton, not to praise him. Their implicit complaint is that Elton's view that Thomas Cromwell created the early modern English state, has repeatedly been questioned for over thirty years and has been shown to be wrong in a variety of ways—but has nonetheless refused to die.

Some twenty years ago G. L. Harriss and Penry Williams attempted in the pages of *Past and Present* to demonstrate that Elton's assertion of a Cromwellian revolution was both overstated and oversimplified. They alleged that Elton had neglected the late medieval antecedents of a developing national church and state and had mistaken for revolution what, in reality, was the resumption of earlier norms in finance and administration. Cromwell, they argued, was a talented administrator at the end of a late fifteenth- and early sixteenth-century interlude of personal rule, but the successful return to national government in the Elizabethan period was principally the result of changing political and social conditions and much the independent work of men such as William Paulet, Francis Walsingham, William Cecil, and Robert Cecil. The impact of Cromwell was not to be minimized, but neither was it pivotal.

Elton's response to this and other criticism has been to qualify some of his original assessments, to admit to some underestimating of antecedents and overstating of results, and specifically to concede that he had credited Cromwell with too much

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"planned consistency" and too much success in his attempted reforms. But Elton has remained steadfast in his characterization of revolution in the 1530s and of Thomas Cromwell as the conscious architect of a national bureaucratic state. He has insistently maintained that Cromwell's achievement lay both in his vision of reform and in the ultimate realization of all that he had intended.

A new attempt to deny the existence of a Tudor revolution, the present book emerges from the research of its six contributors—David Starkey on the Privy Chamber, Jennifer Loach on Parliament, John Guy and Dale Hoak on the Privy Council, and J. D. Alsop and Christopher Coleman on the instruments of Tudor finance. Instead of dwelling on the earlier charge that what Elton found in the 1530s was not entirely new (a point that Elton has always acknowledged), this volume focuses on the continuing importance of household government throughout the 1530s and, further, seeks to demonstrate that the major changes of the sixteenth century were neither the work of a single intelligence nor the result of a visionary design. They were rather, Starkey concludes, "the outcome of the most complex interplay of circumstances, over which chance and the luck of the political game preside[d] as often as reason or will" (p. 201). According to this interpretation, Elton is too whiggish, too short-sighted on the complexities of change; he fails to account sufficiently for pragmatism and politics and exaggerates the role of principle and plan.

The corrective offered by this collection is valuable. The contribution is based on new research that widens the angle of vision beyond Cromwell and the 1530s and by so doing fills in much of the texture and shading absent from Elton's original bold outline. Whether it destroys that outline is another matter. Like most revolutions that have endured, Elton's adapts well to criticism and change. His answer to the question of when and how medieval English society, characterized by personal, household-centered control of administration, underwent transition to a modern national bureaucratic state may no longer be definitive, but it still remains credible, and in many respects it is still persuasive.

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DAVID ZARET. *The Heavenly Contract: Ideology and Organization in Pre-Revolutionary Puritanism*. Chicago: University of Chicago Press. 1985. Pp. x, 214. $22.50.

Into the maelstrom churned up by Max Weber's *The Protestant Ethic and the Spirit of Capitalism* sails David Zaret in an always stimulating if at times unpersuasive book. Unlike Weber, who supposed that the Calvinist doctrine of predestination generated behaviors that in time encouraged economic rationalization, Zaret considers covenant theology the mediating link between capitalism and faith, and, finding previous explanations for its increased importance at the end of the sixteenth century insufficient, he locates its provenance in an "organizational dilemma" troubling the Puritan clergy (p. 14). Members of a national church whose authority they acknowledged but whose worship they wished to change, these preachers confronted bishops hostile to their ecclesiastical reforms and lay dissenters urging them toward more radical designs. Frustrated by the episcopal hierarchy but unwilling to forsake the church, eager to mobilize godly parishioners but fearful of letting loose the dogs of heresy and unrest, Puritan ministers after 1590 turned their attention to deflecting the energies of a critical, articulate laity into personal rather than institutional reformation. Covenant theology provided an appropriate medium, for it made plain the ways of grace by employing contractual language familiar to an audience of yeomen and artisans enmeshed in a burgeoning market system. Zaret embraces Weber's general hypothesis concerning the "intimate relationship" (p. 206) between economic and religious rationalization but stands the master on his head: the spirit of capitalism preceded, not followed, the Protestant ethic.

Advancing the notion that a movement's organization "mediates between its beliefs and its socioeconomic environment" (p. 129), Zaret illuminates the multiple factors that determined the development of covenant theology. Responding to organizational pressures by adverting to doctrinal precedents interpreted in the light of established economic behavior, ministers elaborated on existing covenantal formulations to keep their followers in line. Zaret smartly avoids reductionism, acutely discusses the preconditions for lay dissent, and heightens our appreciation of covenant theology's political ramifications. Nevertheless, the analysis has weaknesses because Zaret overemphasizes the primacy of the covenant in Puritan theology and because, while probing the social sources of ideology, he does not examine lay behavior to test that ideology's social consequences. Though an important complex of ideas, covenant theology controlled neither clerical thinking nor lay expression. Believers described their religious experiences as submissions to God rather than compacts with him, diction suggesting that, however much preachers may have clothed Calvinism in contractual idioms, men and women did not perceive grace as a negotiated settlement. Zaret's conclusion that covenant theology contributed to the rise of possessive individualism also seems problematic when one observes Puritan arti-

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sans at work. The Protestant ethic helped Nehemiah Wallington—Paul Seaver's London turner—to survive (if not prosper) in a precarious marketplace, but Wallington was no entrepreneur on the make. Diligent labor, he believed, glorified God, beat down the flesh, and joined him to the community of saintly craftsmen. These criticisms aside, Zaret's study opens a promising perspective on a most pertinent question: why did Calvinistic Protestantism first develop an extreme form of introspective zealotry in Tudor-Stuart England? The answer, when it comes, may involve the ministry's varied political circumstances throughout the world the Reformers made.

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JAMES SUTHERLAND. *The Restoration Newspaper and Its Development*. New York: Cambridge University Press. 1986. Pp. ix, 262. $30.50.

Rather a genial miscellany than a critical history, this book deals primarily with London newspapers from 1660 to about 1720, leaving the earlier field to Joseph Frank's *Beginnings of the English Newspaper* (1960) and the provinces to Roy M. Wiles's *Freshest Advices* (1965). James Sutherland writes with competence and ease, quoting liberally as befits a man who has edited the *Oxford Book of English Talk* (1953) and the *Oxford Book of Literary Anecdotes* (1975). His initial chapter on the origins and development of English newspapers is admirably clear, and his successive chapters on the coverage of London news, country news, and foreign news are comfortably detailed if unastonishing. The penultimate chapter, on the political positions and domestic censorship of the English press, will be of most use to scholars, although here Sutherland's most interesting sections draw on the work of Raymond Astbury, Timothy Crist, and Michael H. Harris. Sutherland is better as a gleaner than as a theorist; he is more the annalist than the analyst.

I doubt that this book will reach a large audience, but it may serve as grist for the mill of social historians, media scholars, and sociologists who have not the extreme patience requisite to the reading of the chiefly official gazettes that appeared during the Restoration era (with the brief and exciting exception of the years 1679–82, during which the Licensing Act lapsed). Sutherland presents material that is apt to discussions of literacy in early modern Europe, the role of news in urban and rural societies, the growth of reporting, and the power of the press to effect change.

Sutherland inveterately sidesteps controversy and refrains from significant conclusions. His efforts have been sustained by "an admiration (at times reluctant) for the way in which those early journalists resisted all attempts to suppress their papers" and "a growing conviction that the last decades of the seventeenth century, and more especially the heady years from 1679 to 1682, were a formative period in the long history of the English newspaper" (p. 232). One would do better, on the first account, to read Stanley Harrison's *Poor Men's Guardians: A Record of the Struggles for a Democratic Newspaper Press, 1763–1973* (1974). On the second account the reader should turn to the newspapers themselves, many of which are now on microfilm.

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ALAN MACFARLANE. *Marriage and Love in England: Modes of Reproduction, 1300–1840*. New York: Basil Blackwell. 1986. Pp. xi, 380. $24.95.

JUDITH SCHNEID LEWIS. *In the Family Way: Childbearing in the British Aristocracy, 1760–1860*. New Brunswick, N.J.: Rutgers University Press. 1986. Pp. xi, 313. $29.00.

Marriage and Love in England represents the culmination of much of Alan MacFarlane's previous scholarship and is a book of major importance. More significantly, this work offers an analysis and explanation of the uniqueness of the English family. The result is a rich mine of thoughtful reflection and information that should attract the attention of every student of the subject.

To MacFarlane the uniqueness of the English family derives from its deeply rooted fidelity to the Malthusian model, leading him to take for his central task explanations of how the "system worked as a set of interlocked institutions" (p. 45) and when it emerged. Starting from the Malthusian model, MacFarlane develops a system composed of a number of interdependent conditions, primarily the consideration of costs and benefits in deciding when to marry, the influence of romantic elements on choice of spouse, the nearly exclusive control of this decision by the couple, the establishment of an independent nuclear household, the relative insignificance of children's economic contribution to the family, the emotional rewards parents derived from their children, and companionate, egalitarian relations between husbands and wives. The reference to egalitarianism, however, seems to overstate its prominence and to overlook the vast inequalities and disabilities coverture inflicted.

MacFarlane's research supports Malthus's contention that these social conditions developed symbiotically with the rise of capitalism. Others agree. E. A. Wrigley and Roger Schofield have already demonstrated that age of marriage, and consequently size of population, is crucially dependent on the level of

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wages: as wages rise, age of marriage decreases; as they fall and it takes longer to accumulate the assets to support a family, the age rises. Later age in marriage, in turn, truncates the wife's fertile period, and the number of children in the family decreases. With fewer children capital that would otherwise have been required to raise them accumulates and becomes available for investment. Such were the conditions from the sixteenth to the nineteenth centuries in England.

With the possible exception of some parts of Western Europe, America, and Australia, a different family system prevailed in the rest of the world. The central differentiating feature of this alternative, or "familistic," system was the absence of control by the couple over critical decisions in such areas as choice of spouse and age of marriage, leading to inegalitarian and authoritarian spousal relationships. Children's roles in this system were also different; their economic significance was more highly valued, and procreation was often religiously mandated by familistic values.

MacFarlane underscores the social, economic, and political consequences of these contrasting systems. The support for individualism in the Malthusian system promoted the growth of political institutions that protected entrepreneurialism and the growth of capitalism. Calculated self-interest operating as a regulatory mechanism for population expansion tended to avoid both governmental intervention for the same end and the Malthusian checks such as disease, famine, and war. The fruits of the Malthusian system included, according to MacFarlane, "equality of the sexes, physical comfort rather than misery, and responsibility for one's own decisions" (p. 343).

"Tracing the origins of Mathus's elegant theoretical model of the marriage system in England in the early nineteenth century" (p. 5) occupies most of the chapters. MacFarlane first establishes that Malthus's model was an accurate description of the conditions of his day and then deals with each element of the model separately as a historical subject. Most of his evidence is drawn from the sixteenth to eighteenth centuries and, somewhat surprisingly, includes references to conditions in the upper gentry, although he earlier claimed, "we will concentrate . . . on the rest of the social strata" (p. 48). Curiously, the marriage patterns of the propertyless classes are included in the Malthusian model, with its great dependence on a calculated use of property. MacFarlane's thesis would benefit greatly from an analysis of this problem.

The conclusion derived from the evidence locates the origins of the system in the time of Chaucer. Since a key part of the analysis rests on the proportion of the English who were wage earners, the present work is dependent on the validity of MacFarlane's previously presented thesis that no peasant class existed in England (*The Origins of English Individualism* [1978]). That thesis was and remains, however, a matter of controversy among scholars. Nonetheless, the case for Chaucerian roots is often convincingly made, but it is a contribution against which all future research on the English family must be measured.

The upper gentry, largely omitted from MacFarlane's study, are the focus of Judith Schneid Lewis's book, *In the Family Way: Childbearing in the British Aristocracy, 1760–1860*. In contrast to MacFarlane's somewhat static analysis, Lewis seeks to demonstrate that time brought significant change on the issues she examines. Unfortunately, an appreciation of these changes is handicapped by the incompleteness of Lewis's description of the childbearing ethos that prevailed at the beginning of the period she studies. The strength of the book lies in its sympathetic treatment of the evolution of the experience of childbearing, especially in the attention paid to the effects of that experience on the women themselves. Lewis examines fifty wives of barons, viscounts, earls, marquises, and dukes, as well as female members of the royal family, whose diaries and correspondence provide the bulk of the evidence for the book.

Lewis contends that the aristocracy's shift in the beginning of the nineteenth century from an emphasis on public and ritualistic behavior to a system of values highlighting an emotional life and domestic virtues had profound effects on childbearing. From Lewis's perspective childbearing includes not only the subjective experience of pregnancy, labor, delivery, and confinement but also crucial relationships with spouses and medical professionals. Significantly, the eighteenth century saw a shift in the sex of the attending professionals, from female midwives to male accoucheurs.

Over these years aristocratic women continued to regard the production of heirs as their major duty, so that childbearing was viewed by them as an ongoing experience throughout their fecund years. Further, childbearing was consistently attended by the best care that money could buy. In an excellent chapter analyzing the intimate relationship between these women and their accoucheurs, always armed with the latest medical technology, Lewis demonstrates that the painful aspects of delivery persisted unmitigated until the mid-nineteenth century because of prevailing medical philosophy. The philosophical admiration for nature decreed a ban on interference with labor, leaving women to suffer for hours and sometimes days before any assistance was offered. That this suffering conformed to the divine injunction that Eve should suffer in childbirth supported these women in their silent and fearful acceptance of these circumstances. Lewis also sug-

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gests that the pain was a key element in the role that childbirth played as rite of passage to adulthood.

Lewis shows how a compelling sequence of events changed all this. In 1817, following fifty hours of labor and the birth of a stillborn male, the daughter of George IV died. Public outcry at the death of Princess Charlotte was directed at reinstituting an older but short-lived practice of an early medical intervention that relied on forcepts to alleviate difficult labor. Joining in the protest against prevailing birthing practices were influential husbands who were encouraged to be present at delivery by the new domestic values of the nineteenth century. They, too, demanded an end to the suffering they witnessed. The discovery of anesthesia in 1847, and Queen Victoria's acceptance of it, combined with these other forces to change, finally, the whole experience.

Without the element of severe pain labor was no longer viewed as a rite of passage. Nurturing came to replace it as the most significant aspect of womanhood. Domestic values emphasized mothering rather than the ability to bear pain as a characteristic on which women were to be evaluated. Lewis examines the other stages of the childbearing process in several chapters that contain valuable insights into the personal experience of the fifty women. As a result we know far more about the English aristocracy and how it evolved under the influence of domestic values.

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MAX F. SCHULZ. *Paradise Preserved: Recreations of Eden in Eighteenth- and Nineteenth-Century England*. New York: Cambridge University Press. 1985. Pp. xv, 368. $39.50.

Paradise may have been lost with the expulsion from Eden or, as later generations in England believed, with enclosure, industrialism, the growth of cities, the end of one's childhood, the coming of railways, the publication of Darwin, or the liberation of women. And yet, as Max F. Schulz persuasively argues, throughout the eighteenth and nineteenth centuries, the dream of recreating a lost Eden motivated Whig gentleman farmers as well as Victorian architects, romantic poets, and decadent painters and informed landscaping, city planning, poetry, and painting.

In describing these new versions of paradise, Schulz traces a dialectic between the "real and conceptual" (p. xii), the changing imaginings in visual and verbal art of paradise regained and the manifestations of such tropes in material forms. With the onset of industrialism, Schulz demonstrates, paradise moved from the country gardens of the Whig aristocracy to the purely imaginative visions of romantic poets and painters. At midcentury paradise was once again re-created in the garden, now set within the city, the Crystal Palace in Hyde Park. But, with growing despair of achieving such a union of the garden and city, Eden was once again relocated to the realm of art, the bowers of Pre-Raphaelite paintings or the consummate artificial garden, Whistler's Peacock Room.

In contrast to Raymond Williams's *The Country and the City* (1973), Schulz emphasizes the conceptual and imaginative rather than the class and economic context of such representations. The value of this approach lies in its reshaping of intellectual and artistic history as the protean activity of re-creating Eden for one's own time. Read intertextually within this tradition, familiar works take on a new resonance. We see Wordsworth's difficulty in the railway age of locating a material paradise in the "real" Grasmere of his childhood as leading to his internalizing of Eden within memory and, finally, within the Established Church. We also see Victorian works freshly by being made aware of what they exclude from this tradition. Schulz points to Tennyson's rejection in "The Palace of Art" of the material *hortus conclusus* in favor of the painted garden as a continuation of the aestheticizing of paradise. D. G. Rosetti's setting the Annunciation indoors rather than in the traditional garden in his *Ecce Ancilla Domini!* indicates the poet-painter's despair of achieving either an earthly or a heavenly paradise. Yet the book is most valuable when it takes up those few fusions of the imaginative and the material, Edens objectified. The intense wonder and delight of the Victorians at the Crystal Palace of 1851 becomes explicable when the inclusion of oak trees and machine-made commodities under a roof of iron and glass is seen as, for a moment, resolving the contradiction between the new technology of the urban world and the persistent dream of the garden.

The book moves from the failed dream of what Schulz nicely describes as "an *urbus conclusus*" (p. 180) to the despairing return at the end of the nineteenth century to Eden as artifice, epitomized in Whistler's Peacock Room, perceptively termed the *Locus Spiritus* for the times (p. 306). Wrought from conflict between industrialist and artist, it is the ultimate enclosed garden of art, its gold peacocks wholly out of nature, a decadent reprise of the orangerie of the Whig country gentleman.

For all its breadth and erudition, this beautifully produced and illustrated volume is, however, weakened by an uncertainty in the definition of its subject. For, if paradise is defined broadly as that imaginative or material locus in which desire can be fully satisfied, then the range of subject matter—the discussion, for example, of changing representa-

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tions of woman as Eve, Lilith, or Mary—can be justified, although at the cost of a certain looseness. But the volume also contains a thin book within the thick, a valuable counterpart to Leo Marx's *The Machine in the Garden* (1964), tracing the hold on the British imagination of this identification of Eden with the garden. And, in its conclusion, linking the conceptual with the real by connecting the British urge to recreate paradise as a garden with the failure of British industry, this volume makes an important contribution to the current debate over the reasons for the decline of Britain.

HERBERT L. SUSSMAN
Northeastern University

ROBERT W. TUCKER and DAVID C. HENDRICKSON. *The Fall of the First British Empire: Origins of the War of American Independence*. Baltimore: Johns Hopkins University Press. 1982. Pp. viii, 449.

This book challenges the commonly accepted explanatory framework for the origin of the American revolution. Robert W. Tucker and David C. Hendrickson argue that the Whig view has proven so powerful that even "imperial" dissenters have been trapped in its central premises. They assert that English policy after 1763 did not depart from the status quo and that the idea that it did has bound generations of historians to a serious misunderstanding of the events preceding the revolution. The authors claim that the British were perfectly consistent in their view of empire, that no change from mercantilism to imperialism occurred. The key problem was rather the different perceptions of the status quo in London and America.

Tucker and Hendrickson agree with most authorities that the Seven Years' War was a turning point in British-American relations. Their reasoning, however, differs. They contend that the war emphasized the importance of North America to everyone involved, that after the war the French determined to prevent the British from benefiting from their empire, and that the British insisted on maintaining an army in America because of their continuing fear of the Bourbon powers. Most important, the colonists not only were freed from a direct French threat but began to see the French as possible allies against Great Britain.

The logic of these positions became apparent during the Stamp Act crisis, and the American stance remained consistent down to Lexington and Concord. American rhetoric and theory may have changed during the next decade, but the essential American attitude did not. Tucker and Hendrickson see the colonists asserting their own sovereignty, while paying obeisance first to Parliament and then to royal prerogative, neither of which they really accepted. The British leaders, on the other hand, could not accept an empty affirmation of their sovereignty. Declaratory Acts were not enough. For empire to mean anything, Britain's power had to be real. Thus, there was no common ground and no basis for compromise. Force, not concessions, presented the only way for London to win. But a consensus among Britain's politicians made the use of force almost impossible. Although both government and opposition asserted parliamentary sovereignty, both also sought to avoid confrontation, especially armed confrontation, with the colonists. The result was "the worst of all possible worlds for the metropolis," where the British insisted on "matters of form" but "were remarkably pliant in matters of substance" (p. 159).

This book is longer than necessary. Most of the chapter on the Peace of Paris should have been omitted, and repetitious argument could have been pruned. The authors also claim too much originality. Many of their points have been suggested by others. Still, this new "imperial" viewpoint is to be welcomed. By emphasizing the realities of sovereignty and the impossibility of reconciling the colonial and British positions, the authors have returned the discussion to 1765, when Governor Francis Bernard already saw this issue as the unavoidable problem. Unfortunately, the British ministers remained blind to this reality for another ten years.

JAMES L. MCKELVEY
University of Connecticut

CHRISTOPHER REID. *Edmund Burke and the Practice of Political Writing*. New York: St. Martin's or Gill and Macmillan, New York. 1985. Pp. xii, 238. $27.50.

Christopher Reid provides a comprehensive, critical analysis of Edmund Burke's rhetoric in the widest interpretation of the subject. He thoroughly discusses Burke's language (or, better, his several "languages"), his modes of expression, his various methods and styles of exposition and argument, the stylistic relations of particular writings to the context in which they were composed, the purposes intended, and the audience addressed, and the connections between Burke's rhetorical usages and the literary conventions of eighteenth-century political controversy. Reid is principally concerned with Burke as a writer and stylist rather than with his ideas and philosophy, but, by his careful and close reading of Burke's writings and speeches, he significantly contributes to our understanding of Burke's mind, thought, and personality, as well as to an enhanced appreciation of Burke's mastery of English prose. In Burke's impact on the reader his style and language—the metaphors, similies, and allusions—are nearly as important as, and at times

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superior to, the content of his argument. Reid helps us understand why it is still easy to be seduced from one's critical acumen by Burke's rhetoric and to forget how dated, or at least irrelevant to modern conditions, some of Burke has become.

The study is divided into three sections. The first, "The Language of Politics," discusses several "languages" used by Burke in various combinations: the "legal idiom," which infused eighteenth-century political controversy; aesthetic concepts (Burke had originally made his reputation with the *Sublime and Beautiful*) as applied to politics; economic usages, concepts, and methods of exposition; and the various literary styles of patronage, deference, and dissent. The second section, especially important for understanding how Burke composed his famous speeches (and by implication, how he composed in general), is a close textual analysis and comparison of some of Burke's surviving drafts, reports of the speeches as actually delivered, and the revised speeches as published. This section clearly shows how rough notes evolved through delivery in the heat of debate into the subsequently revised and "improved" version created for a larger audience. Finally, the concluding section opens with a comprehensive rhetorical analysis of the pamphlet war engendered by John Wilkes and the Middlesex election controversy of 1768–70 as an essential part of the background for Burke's *Thoughts on the Present Discontents*. Although this pamphlet, like most of Burke's writings, clearly rose above the immediate historical context to elucidate more general problems of politics and philosophy, Reid shows the close connection between the context and the preceding pamphlet debate and Burke's own presentation—how he used the specific incident to rise to and develop general themes and ideas that went far beyond the particular controversy. In Burke, style, content, and context are equal contributors to the whole. Subsequent studies of Burke, whether as a politician, a political philosopher, a writer, or one of the great men of the eighteenth century, will need to pay close attention to Reid's discussion of Burke's rhetoric as a major key to understanding the man and his writings.

ROBERT A. SMITH
Emory University

JONATHAN MENDILOW. *The Romantic Tradition in British Political Thought*. Totowa, N.J.: Barnes and Noble. 1986. Pp. 267. $31.50.

ANDREW VINCENT and RAYMOND PLANT. *Philosophy, Politics, and Citizenship: The Life and Thought of the British Idealists*. New York: Basil Blackwell. 1984. Pp. x, 222. $34.95.

The revival of the study of the nineteenth-century intellectual tradition, firmly grounded in the works of the political thinkers and their historical context, is immensely satisfying. The Victorians suffered first from the socialist historians and more recently from the structuralists. The attempts of these scholars to analyze society in order to reform it were often self-regarding and, frequently, repositories of preselected proofs of bourgeois hypocrisy and of illustrations for ahistorical criticism of industrial capitalism rather than enlightened attempts to understand a novel social environment. Increasingly, however, scholars are recognizing that much can be learned from nineteenth-century political beliefs and practices both about that century and about the present one. Both these books are evidence of that tendency.

Jonathan Mendilow's *The Romantic Tradition in British Political Thought* certainly fulfills the promise of its title. The first three chapters outline the political theories that can be extrapolated from the poetry of Wordsworth, Southey, and Shelley and illustrate them with a rich assortment of quotations. In the fourth chapter Mendilow's real hero appears, Thomas Carlyle. The rest of the book traces the romantics' influence, seen as largely transmitted through Carlyle's writings, on Tory radicals (men as disparate as Randolph Churchill and the Earl of Shaftesbury, with most attention paid to Benjamin Disraeli, Charles Kingsley, and John Ruskin), on early Labour figures (particularly Keir Hardie and Robert Blatchford), and, finally, on the imperialists.

The book covers a great deal of ground in a mere 267 pages, and, therefore, to say that at times the discussion is superficial is hardly a criticism. Occasionally, however, this superficiality leads to an imprecise use of "conservative" and "radical" and a misleading depositing of ideas that were the common coin of the realm to the particular account of the romantics. Charles Kingsley and Benjamin Disraeli uneasily share the same conservative bed with or without a radical cover, and, although John Stuart Mill believed with Southey in an age of transition, Mill derived the idea from Continental sources. Nevertheless, Mendilow's clear introduction to many of the underlying currents common to political writers and reformers of all shades of opinion is valuable even if, in his enthusiasm, he sometimes seems to attribute greater influence to the romantics than the facts substantiate. His discussion points up the complexity of ideas frequently simplicistically categorized and assigned to either the Right or the Left.

Philosophy, Politics, and Citizenship by Andrew Vincent and Raymond Plant has a narrower focus than Mendilow's book, and the discussion is more detailed (sometimes to the point of repetitiveness) and also more sophisticated. It too throws a great deal of

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light on conservatism and radicalism, albeit from a different angle. By their study of the thought of T. H. Green and his disciples, Vincent and Plant illustrate that the assumptions of the liberal and socialist reformers in the late nineteenth century have little in common with those of their similarly labeled successors one hundred years later. And yet many of the principles then established and the reforms enacted in accordance with them continue to be regarded as unassailable "goods" by contemporaries who no longer understand the premises underlying the programs and would reject them if they did.

The authors state their purpose plainly: "The aim of this book is not only to describe and assess Idealism as a form of political theory but also as a 'practical creed' in Sir Henry Jones's phrase, looking at its influence upon the development of 'New Liberalism' and thus on the foundations of social democracy and upon various forms and agencies of social reform such as social work, community work and education" (p. 3). The book follows this plan, beginning with a blessedly lucid exposition of the idealists' debt to Hegel and his influence on the thought of Green. The description of Green's influence on the teaching of the undergraduates at Oxford, particularly at Balliol, in the 1880s (although he taught at Oxford only from 1878 to 1882, his influence continued well after his early death) manages to catch some of the enthusiasm that inspired his younger contemporaries such as Arnold Toynbee. The emphasis, however, is more on the somewhat older men, including R. B. Haldane, Bernard Bosanquet, and D. G. Ritchie as, in the authors' words, "a group of philosophers centered around the pivotal figure of Thomas Hill Green" who "fundamentally changed the conception of the role of the state within the Liberal tradition of political thought" (p. 1). Their philosophy taught that when the state had created the conditions of "positive freedom" for all its citizens by providing housing, education, pensions, and insurance, those citizens (who mostly seem to have had wives rather than husbands) would choose the higher pleasures in life, the chief of which were self-realization through self-improvement and fulfillment of one's duty to society. This belief lay behind the far-reaching liberal reforms at the beginning of the century, which bequeathed to us many of the institutions that form the basic structure of our society. This faith was the English fruit of German idealism; paradoxically, it was supported in many of its proposals by home-grown empiricism. The hopes of neither school have been fulfilled, and yet, after more than half a century of soap over opera and the displacement of duty by gratification, society can think of nothing better than to strengthen the dose and multiply the recipients.

In the contribution to contemporary political thought of the revived tradition of intellectual history exemplified in these two books lies hope for a better understanding of politics and the art of the possible.

ANN ROBSON
University of Toronto

ASA BRIGGS. *Wine for Sale: Victoria Wine and the Liquor Trade, 1860–1984*. Chicago: University of Chicago Press. 1985. Pp. ix, 199. $22.00.

Asa Briggs enhances his short history of the Victoria Wine Company with an "exploratory study" of the British wine trade as a whole. In 1860 William Gladstone persuaded Parliament to encourage the sale and consumption of wine by reducing import duties and ending the jurisdiction of the licensing magistrates over wine sellers. Founded in 1865 by William Winch Hughes, the Victoria Wine Company took advantage of these reforms. It helped pioneer multiple-branch retailing, offering its largely middle-class customers good quality at low prices. Despite its name it stocked more than wine, and in its early years did much of its trade in beer and spirits. Today, as part of Allied Breweries, its more than nine hundred shops constitute Britain's largest wine dealer.

The temperance movement, pressure-group politics, and the brewing industry dominate what little study has been made of drink in Britain. In contrast Briggs celebrates the delights of wine and describes entrepreneurship in a section of the drink trade, which, unlike the public house, British historians seldom mention. Despite a few distinctive details, such as the founder's widow managing the firm for over seven years, the history of Victoria Wine parallels that of other large wine retailers, particularly in its twentieth-century mergers and takeovers. Victoria Wine became a private limited company in 1920, and the brewers Taylor, Walker and Company acquired control in 1929. In the mid-1930s Victoria Wine began to appoint men to manage its shops; previously, women had been in charge.

Although indispensable as a sketch, this book will disappoint historians who admire the many other books by its distinguished author. Inadequate source material often makes anecdote take the place of analysis. Although he supplies bibliographical footnotes and comments occasionally in the text, Briggs omits any systematic description of his sources and thus reduces the value of the book to other historians. He supplements scarce company records with a variety of published sources—newspapers, governmental committees, official statistics—as well as advertisements, photographs, and

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the occasional interview. Probably, he should have done more research in the drink trade press.

Briggs shrewdly asks the important questions, but his answers seldom achieve much depth. He recognizes that "changes in society and culture" explain more about the wine trade than do business strategies but is content to allude to these changes without detailed analysis. Frequently, the proportions in Briggs's book seem odd. For instance, when discussing the late nineteenth-century wine trade he might have devoted more space to W. and A. Gilbey, whose business greatly exceeded that of Victoria Wine. Although Briggs emphasizes the radical changes in the British wine trade since World War II, he compresses these eventful years into little more than an outline.

Briggs surveys an important topic. By 1980 a majority of British adults drank wine at least occasionally, and alcoholic drink of all kinds accounted for more than 8 percent of all consumer expenditure. Competition has reshaped the drink trade. By 1984 the British purchased nearly half their alcohol in supermarkets.

DAVID M. FAHEY
Miami University

J. A. YELLING. *Slums and Slum Clearance in Victorian London*. (London Research Series in Geography, number 10.) Winchester, Mass: Allen and Unwin. 1986. Pp. x, 160. $34.95.

This book is part of a series that seeks to stress linkages between geography and the humanities, including history—a worthy endeavor that is only partially realized in this volume. Readers drawn by the title will be disappointed by the book's scale: it deals neither with "Victorian" London (only the final quarter of the century) nor with all slums and slum clearance (only those touched by the activity of the Metropolitan Board of Works and the London County Council). Strangely, for a geographer, J. A. Yelling ignores broader urban forces creating or clearing the slums—railways, street building, commercial developments, population densities, comparative wage and rent levels, the size and mobility of the London casual labor force—and concentrates mainly on the way the central London municipal authorities implemented the Housing Acts of 1875 and 1890.

Yelling's central thesis is that, by viewing slums as an autonomous problem of morality and health "isolated from economic society and economic geography" (p. 154), the Victorians failed to place slum clearance within the necessary framework of urban planning and "the 'normal' development of the city" (p. 151). The Cross Act (1875) set the mistaken "paradigm strategy" of compensating slum owners and rehousing the working classes on cleared central sites instead of adopting the "market solution" of selling the sites to "higher value users" and building suburban housing estates (pp. 153, 154). In Yelling's harsh judgment the pioneer housing reformers and politicians must be condemned for their "misunderstandings and oversimplifications" and for their "lack of recognition of the true land-use character of slums" (p. 154). The progressives, however, appear as *dei ex machina* with a suburban policy that "involved a distinctly different conception of the historico-geographical development of the slum" (p. 3).

Although this thesis enlivens an often turgid and awkwardly written book, it is a whiggish interpretation that fails to do justice to the complexities of the issue, and Yelling has little tolerance for the system of trial and error that characterized Victorian housing policy. He does not place the symbiotic relationship between the Cross Acts and the model dwelling companies within the setting of a fervent belief in the ability of combined capitalism and charity ("five percent philanthropy") to solve the problem of the central slums. He analyzes neither the mobility of casual labor before the Cheap Trains Act of 1883 and the spread of an extensive tramway network nor the comparative costs of central and suburban housing construction, and he ignores the "leveling-up" philosophy behind the central housing estates and the remarkably innovative housing act of 1885. Above all, he fails to supply a sustained analysis of the viability of the suburb as an alternative to central dwelling for the London laborer.

Yelling has unearthed some interesting new facts, especially on the question of compensation to slum landlords, and he quotes and summarizes well, but much of his material can be found in recent works that he does not cite. Concentrating on paradigms more than complexities, Yelling seems unwilling to accept that history is rarely the story of "straightforward progress" (p. 2) and "spectacular policy initiatives" (p. 31), however regrettable that may be.

A. S. WOHL
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T. R. WRIGHT. *The Religion of Humanity: The Impact of Comtean Positivism on Victorian Britain*. New York: Cambridge University Press. 1986. Pp. xiii, 306. $44.50.

Influence hunting is one of the most nebulous forms of intellectual history. Much depends on the standard adopted. Some twenty years ago W. M. Simon's *European Positivism in the Nineteenth Century* adopted a perhaps too stringent standard. The current tendency is to be more generous, and T. R. Wright's book conforms to this trend. "With the best

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will in the world" Simon declared himself unable to detect much evidence of the celebrated influence of Positivism in George Eliot's writings. Wright, a literary historian more intimate with Eliot's work, and perhaps more sensitive to the resonances of Comte's Religion of Humanity, persuasively demonstrates that the influence was very real. He also shows the considerable extent to which the writings of George Gissing and Thomas Hardy reveal Positivist influence.

As the title of Wright's book indicates, he concentrates on the influence in Britain of Comte's more controversial later work. Although the distinction between the "sane," scientific Comte of the early *Positive Philosophy* and the "crazy," utopian Comte of the *Positive Polity* has been generally discredited, it was influential in the nineteenth century; no less a figure than John Stuart Mill contributed to its propagation. The task of the influence hunter is complicated by Comte's desire for more than mere influence; he wanted faithful disciples, a priesthood ready for the historically inevitable and imminent advent of the Positivist social order. Thus, he anathematized those who presumed to admire his earlier works yet rejected his later ones. With Comte's death, and the indefinite postponement of the advent, his followers had to confront the question of whether to promote the widest dissemination of Comte's ideas, at the risk of contamination and dilution, or to remain few and pure.

In England the Comtists tried, quite understandably, to have it both ways. Wright provides a sound survey of organized Positivism, with particular and welcome emphasis on its liturgical aspects, and shows it to have been more schismatic than charismatic. This helps us to understand its failure as a synthetic religion to attract more than a small, though intellectually distinguished, membership. More successful were the English Comtists' attempts to disseminate their ideas. In this they were helped by the early and respectful attention given to Comte's philosophy in the 1840s and 1850s. The difficulty was to overcome the hostility to the *Polity*. Here, the central figure was Frederic Harrison, a talented controversialist who dedicated himself to harmonizing Positivism with indigenous liberalism in politics and religion and to ingeniously arguing that various eminent intellectuals such as Matthew Arnold, Fitzjames Stephen, and John Ruskin were Positivists without knowing it. As Wright shows, a number of distinguished Victorian broad churchmen responded favorably to Comte's religious ideas, although this may have been because they benefited from a moment of Anglican intellectual vulnerability. Apart from Harrison's polemics, particularly with Herbert Spencer and T. H. Huxley—Spencer suffering, as Wright shrewdly notes, from an extreme case of the anxiety of influence, Huxley being contemptuous of Comte's retrograde notion of science—one is impressed by the serious attention paid to Comte even by those who could not accept discipleship: such intellectual eminences as Leslie Stephen, John Morley, Henry Sidgwick, Mark Pattison, Walter Pater, and Benjamin Jowett. Without being sure what it all adds up to, I am persuaded by Wright's scholarly and thoroughly documented book that the impact of Comtean Positivism on Victorian England was indeed extensive and varied.

CHRISTOPHER A. KENT
University of Saskatchewan

MARY JO WEAVER, editor. *Newman and the Modernists.* (Resources in Religion, number 1.) Lanham, Md.: University Press of America or College Theology Society, Philadelphia, Pa. 1985. Pp. viii, 223. Cloth $24.50, paper $12.25.

In this very fine volume of nine essays, edited by Mary Jo Weaver, we have the first in-depth look at something we have always known—that many of the Roman Catholic Modernists in the early 1900s claimed affiliation with John Henry Newman (1801–90). Their claim became a problem in 1907 when Pope Pius X condemned the Modernists in the encyclical *Pascendi domini gregis* and listed as heresy some positions that looked much like those of Newman, a cardinal of the church. This volume explores the different interpretations of Newman that the Modernists, Rome, and others put forward at the time.

The nine contributors make clear that they have a current theological interest in interpretations of both the Modernists and Newman, an interest made explicit in the final chapter by Gabriel Daly called "A Theological Reflection." In effect Vatican II validated many of the positions of the Modernists that were condemned in 1907 and indirectly accepted the Modernists' claim of descent from Newman. Daly's rather strident language demonstrates his fears of a conservative reaction against Vatican II.

The first eight essays are detailed case studies of the history of the theological interpretation of Newman. Four of them carefully study the Modernists William J. Williams, George Tyrrell, Henri Bremond, and Alfred Loisy. Two others study the biographer of Newman, Wilfred Ward, one examines the Vatican's interpretation of Newman, and the first essay offers a present-day interpretation of Newman. The book would be stronger if, instead of the second piece on Ward and the present-day interpretation, it included an essay on the Italian Modernist Romolo Murri and a full essay on Bishop Edward O'Dwyer, who defended Newman's orthodoxy to the Vatican's great delight.

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The volume shows that the debate turned on how to interpret three proposals associated with Newman: that the doctrine of the church develops (from his *Essay on the Development of Christian Doctrine* [1845]), that humans depend on an "illative" sense akin to what we would call intuition or experience (from his *Essay in Aid of a Grammar of Assent* [1870]), and that scholarly inquiry should be open. The Modernists saw Newman as an alternative within the church to official scholasticism, whereas the Vatican believed that Newman accepted safeguards of orthodoxy that the Modernists did not. The volume reminds us of how central debates over historical understanding are to modern consciousness in the West and of how the losers in one generation may become the winners later on.

C. T. MCINTIRE
University of Toronto

DOUGLAS J. NEWTON. *British Labour, European Socialism, and the Struggle for Peace, 1889–1914*. New York: Clarendon Press of Oxford University Press. 1985. Pp. xiii, 378. $45.00.

The purpose of Douglas J. Newton's excellent study is to "review the policies and activities of the British labour movement in relation to the issues of war and peace during the years 1910 to 1914" (p. 4). The book concentrates on the attempts of the socialists in Britain, in cooperation with socialists on the Continent, to prevent war. Earlier studies of the Second International by James Joll and Georges Haupt have presented the main lines of this sad story. Newton offers no dramatic changes in interpretation, but he adds rich detail, based on careful and extensive research, concerning the antiwar efforts of the British socialists.

In the campaign against the growing militarism of the major European nations, the socialists in Britain faced both internal and external obstacles. Within Britain their main task was to mobilize the trade unionists, who had joined the socialists in forming the Labour party. Through an examination of the records of local trade union councils, the annual meetings of important trades such as the railway workers, carpenters, and miners, and the national Trade Union Congress, Newton shows the limited impact of the socialist efforts. The socialists' campaign against militarism was blunted by the limited scope of traditional trade union concerns, the loyalty of the members to reigning values of British society, and, at times, worries about unemployment in the war industries. The socialists were never able to persuade the trade unionists to assign a high priority to the struggle against militarism.

Division among the socialists was another problem. The blend of Marxism and Christian pacifism from which the largest of the socialist groups, the Independent Labour party, drew its approach to international relations collided with the Marxism of Henry Hyndman and his Social Democratic party. Indeed, the realism that Hyndman derived from his mixture of Marxism and nationalism provided, at least in the light of recent German historiography, a much more accurate assessment of the nature of the war danger. But Hyndman's campaign against the German menace, supported by the most popular of the socialist journalists, Robert Blatchford, weakened the British voice within the Second International.

That voice had never carried much weight compared to the Germans, who dominated the International. And their relationship with the German Social Democratic leaders most frustrated the British socialists in their efforts to develop a common policy. Newton describes the growing mistrust between the socialist leaders of the two countries as the German Social Democrats, fearful of the repressive powers of the Wilhelmine state and mindful of the patriotic sentiments of their own rank and file, drew back from the antiwar strategies developed by Keir Hardie and Jean Jaures. The most promising plan, that of a general strike to be carried out if war threatened, found little support among the Germans.

Newton defends Hardie and Jaures against those who have seen the plan as naive and impractical. The two leaders, he argues, suffered from no illusions about the practicality of the scheme for a general strike. A matter of "bluff and bluster" (p. 293) to restrain the decision makers in a time of crisis, the general strike was the resort of men fully aware of their lack of mass support. The socialists had, in Newton's words, "lost the contest for the hearts and minds of the people," including "many of their own people" (p. 341).

STANLEY PIERSON
University of Oregon

GEORGE L. BERNSTEIN. *Liberalism and Liberal Politics in Edwardian England*. Winchester, Mass.: Allen and Unwin. 1986. Pp. xviii, 247. Cloth $34.95, paper $12.95.

The fate of nineteenth-century political liberalism is a source of continuing fascination to historians. They cannot decide whether liberal parties were dying a strange death before 1914, whether World War I killed them, or whether changed economic and social circumstances transmuted them into socialist or conservative forms. The Liberal party of the first industrial nation and mother of parliaments was a model for liberals elsewhere. In 1914 the Liberal party governed Great Britain and its empire.

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Five years later it was no longer even the second party in a traditional two-party system, and it never again formed a government, although it might for another decade or so manage to destroy one.

In this book George L. Bernstein examines the political health of the British Liberal party from about the turn of the twentieth century until the outbreak of World War I. He begins by describing the party's traditions, leadership, MPs, constituencies in the country, and organization. He evaluates the role of the Liberal Imperialists, led by former Prime Minister Lord Rosebery, and concludes, probably correctly, that they had little chance of capturing control of the party. Bernstein then traces the revival of Liberalism, in uneasy agreement with Labour, that culminated in victory in the general election of January 1906. Finally, he chronicles the achievements, disappointments, and frustrations of the Liberal government during the next eight and a half years.

Bernstein uses his narrative to convey his interpretation of the fate of the Liberal party on the eve of World War I. He favors the view of such historians as Henry Pelling that the liberalism of the middle class was losing its appeal for the working class, and he refutes the argument, espoused by Peter Clarke and H. V. Emy among others, that the Liberal party succeeded in winning over the worker. The author, however, cautiously accepts some ideas on both sides of the question. He admits that Liberalism held its own before 1914, but he contends that it was able to do so because class interests did not dominate British politics at the time. He suggests that neither the new Liberalism, which advocated a limited measure of social reform, nor the progressive alliance between Liberals and Labour could for long prevent the working class from seeking its own political aims separate from the middle class. Less convincingly, he argues that the emotionalism and selfishness of modern politics were bound to destroy a liberal ideology based on reason and moderation. Thus, although he does not state it explicitly, Bernstein seems to believe that the Liberal party was doomed even without the events of 1914–18. His thesis is a rejection of Trevor Wilson's contention in *The Downfall of the Liberal Party* (1966), a work he does not cite, that World War I caused the demise of British Liberalism.

Bernstein's contribution to this debate is valuable not only because he summarizes much of the historical literature on his subject but also because he uses primary sources to explore the issues at both the national and local levels. Of especial interest is his mining of the Liberal newspapers in Leicester, Leeds, and Norwich and of some Nonconformist journals. By doing so he is able to argue that Liberals in the constituencies were not true converts to a new liberalism, were suspicious of such working-class demands as a national minimum wage and the right to work, and were jealous guardians of their hold on political power. In brief, this book provides some insight into the decisions of individuals and groups that both delayed and precipitated the decline of liberalism.

MARVIN SWARTZ
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DANIEL SILVERFARB. *Britain's Informal Empire in the Middle East: A Case Study of Iraq, 1929–1941*. Foreword by MAJID KHADDURI. New York: Oxford University Press. 1986. Pp. x, 200. $24.95.

As the title implies, this book falls within the genre of traditional studies of empire and methods of imperial control. Daniel Silverfarb examines the attempt of a declining imperial power with diminishing economic resources—Great Britain after World War I—to ensure its imperial interests in Iraq in the face of rising anticolonial sentiment.

The book consists of fourteen relatively short chapters on Britain's relationship with Iraq from 1929, when Britain prepared to end its mandate and enter into a new treaty with Iraq, until 1941, when the alliance broke down and Britain had to reoccupy the country temporarily. In the introduction Silverfarb contends that Britain terminated its mandate earlier than necessary and withdrew its forces "without first insisting on adequate safeguards for imperial British interests in the region" (p. 10). The book then shows how the Anglo-Iraq Treaty of 1930 failed to protect those interests, particularly the security of the airbases that were the empire's main communications link, and how the British used the Assyrians, a Christian minority, to guard these bases partly at the expense of the community's future political well-being. Other chapters deal with the impact of Britain's role in Palestine on Iraq, Iraq's relations with Kuwait, Britain's role in supplying arms and credit to Iraq, and the oil concession, which he claims satisfied neither Britain nor Iraq. Silverfarb attributes the deterioration of relations between Britain and Iraq that lead to the outbreak of hostilities in 1941 to several factors including the Palestine problem, the emergence of an anti-British faction in the Iraqi army, and Britain's military reverses in 1940. Because of this breakdown, Silverfarb concludes that informal empire failed. To secure their alliance with the Iraqi ruling class, the British had to make so many concessions that they sacrificed their interests. Silverfarb, however, consistently underestimates the anti-British feeling in the country. Given the continuous nationalist opposition to the mandate and the treaty, starting with the revolt of 1920, the wonder is not that the British failed to secure their interests but

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that they did so for so long (until 1958) and so inexpensively.

The book, drawn from meticulous research in the British archives and using newly opened documents, will be useful for European historians interested in a good case study of informal empire. The same will not be true for scholars of the Middle East. Aside from one work in Arabic, the author relies heavily for material on domestic Iraqi politics on a few well-known writers, such as Stephen Longrigg, Majid Khadduri, Edith Penrose and E. F. Penrose, and, more sparingly, Hanna Batatu. As a result, the book breaks no new ground in the area of British-Iraqi relations although it adds some interesting details to a story that has often been told.

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WESLEY K. WARK. *The Ultimate Enemy: British Intelligence and Nazi Germany, 1933–1939*. (Cornell Studies in Security Affairs.) Ithaca: Cornell University Press. 1985. Pp. 304. $32.50.

Wesley K. Wark has written an excellent book. His object is to set out and analyze the role played by the British intelligence services in the decade of appeasement. The result is a highly technical work, intended for the specialist only, and a major, indeed an indispensable, contribution to the literature. He deals with Air Force, War Office, and Admiralty intelligence in turn and also with the activities of the Industrial Intelligence Centre. His general conclusion is that in the early years of the Nazi regime the British intelligence services displayed a general lack of conviction about the danger of an Anglo-German conflict. Later, when worst-case situations were accepted as fact, the Nazi threat was inflated, and this led to the pessimism in the services that bolstered the appeasement policy of Neville Chamberlain's cabinet. Wark emphasizes that the services did not work together in this sphere, and he concludes that the British intelligence effort of the time was fragmented and uncoordinated.

Air Force intelligence activity was dominated by the fear of a massive German air attack, the knockout blow, designed to cause surrender through a collapse of civilian morale. By the autumn of 1936 the air staff concluded that Britain was losing the air arms race, and by the time of Munich the Germans were thought to possess an overwhelming superiority in the air. Wark reveals that the War Office, like the Air Ministry, suffered from an early and grievous misperception of the dynamism of German rearmament in the Nazi period. At first, the soldiers looked on the leaders of the German army as a stabilizing factor in German politics. The German military professionals seemed to be less ardent and less dangerous than the Nazi politicians. After 1936, when the thirty-six-division limit, announced by Hitler himself, was broken, Britain's military experts began to look on the German army as a threatening arm of Nazi expansion. At the Admiralty a different view prevailed. There, intelligence officers believed that Hitler wanted a new German fleet for use in the Baltic against the Russians. Britain's admirals were convinced that Germany was not building a fleet to be deployed against their country. This opinion was not given up until 1938, when the Germans announced their intention of raising their quota of submarines to 100 percent of the British figure. The Industrial Intelligence Centre, the fourth institution studied by Wark, early decided that the Germans under Hitler were planning for a total war.

Wark's book is based on extensive research in official archives and in a number of collections of private papers. He demonstrates, throughout, a command of the published literature. In chapter 7 he presents a most interesting analysis of the work of Nicholas Kalder, Burton Klein, Alan Milward, and others who have dealt with these subjects. Reviewers sometimes suggest that the books they review are too long and should be cut down. This one would be even better if the author had written at greater length. His conclusions are sober and mature.

ALFRED GOLLIN
University of California, Santa Barbara

MICHAEL J. COHEN. *Churchill and the Jews*. London: Frank Cass; distributed by Biblio Distribution Center, Totowa, N.J. 1985. Pp. xix, 388. $25.00.

This book is the result of a meticulous study of Winston Churchill's statements, public and secret, for references to Jews and Zionism. Michael J. Cohen follows Churchill's political career from the beginning of the century to 1949. According to Cohen, Churchill's attitudes and policies were determined frequently by cynicism and pragmatism and principally by his perception of British interests. He never acted out of compassion and rarely out of humanitarian considerations. For example, his support for Zionism after World War I resulted from his view that it provided an antidote to bolshevism and might help mobilize the U.S. government on Britain's side. Above all, he considered Zionism a means to extend imperial influence and an instrument that might be of assistance in reducing foreign expenditures. The author calls attention to even the most minute manifestations of Churchill's views, even when they were based on blatantly superficial assessments. (In 1935, for instance, after only a forty-eight-hour visit to Palestine, Churchill put for-

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ward an "expert" opinion, which failed to perceive signs of the coming Arab revolt that erupted the following year.)

Cohen's main assault is aimed at Churchill's record during World War II. Having condemned the White Paper of 1939 while in the opposition, Churchill was then called on to prove—in deeds—his belief in Zionism and his compassion toward European Jewry under Hitler. Here, the author accuses Churchill of bad faith. Neither his public declarations nor his private promises to Jewish leaders were translated into action. He neither abrogated the White Paper nor mounted rescue operations. His pro-Zionist line was dictated by cold calculations, such as the ability of American Jewry to hasten the entry of the United States into the war and the possibility of the *Yishuv* relieving the British garrison by providing an independent military force. These considerations alone account for his readiness to grant the Jews a small state to end their sufferings.

As much as Cohen undermines Churchill's Zionism and pro-Jewishness, he still admits that Churchill was the Jews' last friend. Alas, this is a rare favorable remark. Even Churchill's support for the Philby Plan (selling Palestine to the Jews for 20 million dollars in exchange for appointing Ibn Saud as the king of a Middle Eastern federation) is here interpreted as a mere sweetener in view of the suspension of the Jewish division plan. After Moyne's assassination on November 6, 1944, Churchill never again wished to see Chaim Weizmann, and Churchill almost certainly would not have actively supported a Jewish state after the war had he been returned to office in the elections of 1945.

The Holocaust provides the author with his last straw. Here he rejects Bernard Wasserstein's and Martin Gilbert's praise for Churchill's willingness—unique in the British establishment—to do something for the wretched Jews facing the gas chambers. Mercilessly, Cohen sets out to prove that there was not much behind his so-called readiness to help. On so fateful an issue as the bombing of Auschwitz the most powerful British prime minister relegated responsibility to Anthony Eden. In so doing, he accepted the policy of the Foreign Office (no deal with Hitler and no diversion of military resources). Churchill similarly adopted the recommendations of the Foreign Office in connection with the Joel Brand mission.

As much as one wishes to sympathize with Cohen's moralistic judgment, one wonders whether he has not demanded too much of his subject. After all, it is difficult to believe that any other British prime minister would have behaved differently. From a methodological perspective, Cohen should have contrasted not Churchill's public statements with his deeds and private utterances but his overall conduct with that of other British and non-British political leaders. The assistance granted to the rebellious Poles in 1944 shows the relative weakness of the Jews not Churchill's preference for the Poles. Indeed, Jewish impotence, not Churchill's, dictated Britain's conduct toward Zionism and the Holocaust. Thus considered, "the greatest Englishman in our century" still emerges far better than any of his compatriots who held positions of responsibility during the period under discussion.

JOSEPH HELLER
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DAVID GEORGE MULLAN. *Episcopacy in Scotland: The History of an Idea, 1560–1638*. Edinburgh: John Donald; distributed by Humanities, Atlantic Highlands, N.J. 1986. Pp. vii, 279. Cloth $45.00.

David George Mullan has entered a truly ancient controversy, one virtually as old as the institutions it is about. The place of episcopacy within the Scottish Reformation remains as much an issue today as it was in the sixteenth century.

Since the 1940s Gordon Donaldson has spoken outstandingly for the episcopalian tradition. English and Scottish reformers, he argues, were broadly of a piece in that they all looked to the godly prince as the most desirable agency of reform and anticipated some sort of episcopal church government. The Presbyterian movement, with its clericalist rather than royalist preoccupations, arose in the 1570s from Continental, academic sources and constituted a major departure from reformers' original aims and expectations.

Recently, James Kirk has challenged Donaldson's conclusions. Conciliarist assumptions, he contends, informed the Scottish Reformation from the outset, and Presbyterianism grew naturally from the indigenous dynamics of reform. In Kirk's version, the Presbyterian, "whig" historiographical traditions are substantially correct.

Although Mullan views this dispute from an episcopalian perspective, he nevertheless agrees that the overseeing "superintendents" and "commissioners" of the early Protestant church significantly differed from Elizabethan bishops. They neither participated in an apostolic succession nor were consecrated through any sacrament but were simply elected. They were also subject to the General Assembly and in theory were preaching ministers no less than visitors. But Mullan emphatically insists that none of the reformers opposed bishops in principle and, pace Kirk, that superintendents were no short-term expediency but an integral part of the reformers' objectives.

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Whatever the theory, reformed bishops resembled the unreformed variety in many ways, and many incumbents clearly thought of themselves in terms of traditional episcopal governance. Their outstanding weakness was at once fiscal and political. The power of the magnates ensured self-serving appointments. If the new bishops were unarguably better than their unreformed predecessors, they still fell short of the new standards. Even the ablest among them—the wily Patrick Adamson—was no spiritual leader. As a result, antiepiscopal sentiment emerged early and subsequently merged with the new ideology.

James VI's successful imposition of episcopacy in the early seventeenth century did not improve the situation. If his appointments were frequently better than the early Protestant bishops, the new bishops usually found themselves unhappy with the ecclesiastical policies emanating from London, and this increasingly became their dilemma. They were made the reluctant agents of an alien intrusion. Their dependence on the crown and, still more, their commitment to royal authority made them unable to resist or to translate their own authority into real leadership. They never produced a consistent or even fully articulated justification of their office within the Scottish context. Despite their control over the press, the intellectual initiative remained with their Presbyterian opponents. When the revolution came, a surprising number were prepared to accept the authority of the General Assembly.

Interesting as it is, Mullan's study nevertheless suffers from its narrow, ecclesiological focus. The disputes over church polity are closely rehearsed, and in this circumscribed framework a complex figure such as Hume of Godscroft emerges simply as the "presbyterian hatchet man" (p. 163). So, too, we lose the texture of Bishop Patrick Forbes's profound apocalypticism and philosemitism.

In discussions of the power of ideas in Scottish history, Presbyterianism must surely dominate. On Mullan's telling, episcopacy always remained tentative, inchoate, and relatively inarticulate. Even so, a penetrating assessment of the culture of the Scottish Reformation will require us to look beyond the tired, ecclesiological controversies.

ARTHUR H. WILLIAMSON
New York University

BRUCE GALLOWAY. *The Union of England and Scotland, 1603–1608*. Edinburgh: John Donald; distributed by Humanities, Atlantic Highlands, N.J. 1986. Pp. vii, 197. $39.50

Before 1560 England and Scotland were two of the most hostile nations in Christendom. They were fully united only in 1707, and not until 1745 had England fully subdued the Scots to create "Britain." Yet scholars have followed contemporary politicians and publicists who, from the seventeenth to the twentieth centuries, have presumed that unification was inevitable and generally beneficial to both parties. Just as Wales had earlier been merged and submerged to England, Brittany to France, Aragon to Castile, and so on, so Scotland was destined to be united with and annihilated by England. Since most surviving documents are "Anglocentric," they have sealed the case.

Bruce Galloway focuses our attention on the "Union project" of 1603 to 1608, which attended the accession of James Stuart to Elizabeth's throne. In his detailed study of the political development, Galloway dismisses the thesis of inevitability at the outset to emphasize the contingent nature of this political process. Acknowledging the paucity of Scottish documents, he establishes the Scottish viewpoint with sufficient force to balance the more conventional English version of events.

The book develops the story chronologically. In spite of his introduction, which promises to consider "constitutional and governmental factors," "law and justice," "religion and the church," and even "society and economy" (pp. 2–6) and in spite of a statement in his conclusion that he has been "isolating the main factors working for and against success" (p. 172), Galloway has written a strictly political account, which refers occasionally to the cultural prejudices that separated the Scots from the English. Galloway's key chapters are labeled with dates: "March 1603–July 1604," "July–December 1604," "January 1605–December 1606," and "November 1606–July 1607." To be sure, he intersperses chapters on tracts and treatises and projects outside parliament and an appendix on the Union Jack flag. But this is political history that delineates the respective positions of James VI/I, the English Parliament, the King-in-English Parliament, the Scottish Parliament, the King-and-Scotland, and Union-and-the-Kirk.

The strength of this book lies in Galloway's convincing refutation of the validity of the question, "So why did not the union take place?" Arguing that this question is misconceived, he says that sufficient pressure to unify the two nations did not exist. Nor should it have existed. The king, who was not altogether committed to union, the English Parliament, which actually killed the enterprise for the time being in 1607, the Scottish people and government, which did follow their king, and general cultural, economic, and social conditions should not be blamed. The union simply did not happen. It is "no fault" nonmarriage.

The minor flaws in this book that reflect the disparity between intent (to be broad) and execution (to describe day-to-day events exhaustively) stem

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from the author's inability to broaden the scope of his doctoral dissertation. According to a prefatory note by John Morrill, Galloway died in tragic circumstances while he was preparing this work for publication. This fine book shows that Galloway was a meticulous scholar who had a powerful mind to formulate new ideas based on the evidence available. This book is a fine monument to a budding scholar.

DAVID R. STEVENSON
Kearney State College

GUENTER B. RISSE. *Hospital Life in Enlightenment Scotland: Care and Teaching at the Royal Infirmary of Edinburgh.* (Cambridge History of Medicine.) New York: Cambridge University Press. 1986. Pp. xiv, 450. $49.50.

Doctors have long been one of Scotland's most successful exports, produced in much larger quantities than the native population requires. Guenter B. Risse studies the historical epicenter of this phenomenon at the time of its emergence in Edinburgh in the last quarter of the eighteenth century. Although the Royal Infirmary catered to a population of only seventy-five thousand people, as compared with the institutions of London or other European capitals ten times the size, it established an international reputation for practical teaching, for it was closely associated with the fortunes of the university's rising medical faculty.

Risse addresses the problem that has created much division of opinion among historians of whether the old hospitals were as likely to kill as cure their patients. His conclusions on Edinburgh are optimistic: the number of deaths in the infirmary was always small in the eighteenth century, even though, unlike some of the English voluntary hospitals, Edinburgh admitted patients suffering from fevers and other infectious diseases. Reasonable standards of food, warmth, and comfort, combined with the reluctance of William Cullen and James Gregory to use the most extreme type of medical treatment, allowed the patients some chance of relief. Risse views the discharge registers with proper skepticism, since the terms "relieved" and "cured" were used loosely, with one eye on the hospital's patrons, and untreatable patients were not likely to be admitted in the first place. Nevertheless, the hospital was far from being the plague-pit so often imagined by later generations.

That general argument, however, does not have a large place in Risse's book. Apart from the comments on hospital mortality and a brief attempt to place Edinburgh in a European context, Risse lets the records speak for themselves in re-creating the atmosphere of the hospital. He rejects the temptation to use modern ideas to interpret eighteenth-century case records and describes them simply in terms of Cullen's contemporary nosology. The book thus presumes some knowledge of medical practices in the Enlightenment period, since Risse gives little indication of how far Edinburgh accepted, or deviated from, contemporary ideas. He stresses Cullen's empirical and self-critical approach both to medicine and teaching; few teachers at any time have been so prepared to expose the limits of their knowledge to their students, as is revealed in the students' notebooks. A more comparative approach would have given these comments, interesting in their own right, a wider significance.

Risse gives a fascinating and well-written description of the day-to-day workings of an eighteenth-century hospital. His most important source, 808 case histories of individual patients, allows detailed consideration of their diseases and treatments. From this he describes a hospital whose patients were mainly young, with a substantial proportion of seamen, soldiers, and domestic servants, whom the hospital accepted on a contract basis. For the same reason, patients with venereal disease were admitted, although this ailment was usually shunned by British charitable hospitals. Risse's chapters on patients, staff, treatments, and results are readable as well as informative; he has done the magnificent collection of records full justice.

M. A. CROWTHER
University of Glasgow

ALEXANDER FENTON. *Scottish Country Life.* Reprint. Edinburgh: John Donald; distributed by Humanities, Atlantic Highlands, N.J. 1976. Pp. ix, 255. £7.00.

Alexander Fenton defines the major theme of this book as concern with the "material culture of the countryside—the objects people made and used, the ways in which they used them, the houses in which they lived" (p. 226). The definition encapsulates both the strengths and weaknesses of his work. On the positive side, the book details the changing agricultural methods applied in Scotland from the Bronze Age to the early twentieth century, although the main emphasis is on the period from 1600 to 1900. Fenton examines regional differences in cropping and in techniques of husbandry and meticulously recounts the ways farmers and workers used tools and machines. Daily diet, accommodation, and the development of farm transport systems also receive attention.

But this highly detailed approach has the disadvantage of concentrating more on the artefacts of the countryside and on farming techniques than on

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the life of the people. All too often they are portrayed as passive figures on the landscape—as mere operators of machines or consumers of the food and drink described—rather than as individuals with opinions and aspirations.

What is more serious, Fenton's method leads him to neglect the great social and economic upheavals of Scottish rural history during this period. The bitter events associated with the Highland clearances and with the crofters' "land war" of the 1880s are barely mentioned and nowhere analyzed. Because the book was published a decade ago, recent research, for example on farm service, cannot be taken into consideration.

As a record of basic farming practice in Scotland, particularly in the eighteenth and nineteenth centuries, this book can be recommended. And in that connection its copious use of photographs and diagrams to illustrate many of the techniques it describes is particularly useful. But as an examination of the nature of country life or of the structure of the rural economy (and especially of the great changes that occurred in the Highlands during the nineteenth century), it is much less successful. Indeed, Fenton's neglect of the disturbing events associated with the clearances and the crofter unrest and his failure to examine the large-scale migration from the countryside that occurred during the Victorian era results in a misleadingly bland account of daily existence in rural Scotland. Thus, Fenton describes agricultural improvements as giving the country a "face-lift" that was "so all-embracing that it swept away most of the older habitations" (p. 181). Yet no mention is made of the economic and social dislocations that accompanied this, particularly when it was foisted on a reluctant and hostile peasantry, or of the justification put forward by landlords and their agents for the changes they implemented. The addition of a modicum of analysis would have substantially increased the value of this book.

PAMELA HORN
Oxford Polytechnic

MICHELINE KERNEY WALSH. *"Destruction by Peace" Hugh Ó Neill after Kinsale, Glanconcadhain 1602–Rome 1616*. Armagh, Northern Ireland: Armagh Diocesan Historical Society. 1986. Pp. xvi, 434. £30.

Hugh Ó Neill, earl of Tyrone, remains one of the most enigmatic figures of late sixteenth- and early seventeenth-century Irish politics. At times he played the role of worldly Renaissance prince; at other times, that of a tribal leader calling on traditional loyalties. Was he a loyal son of the church, a leader of the Counter Reformation, or was he a man indifferent to religion who was willing to seek aid from whatever source, secular or ecclesiastical? Was the so-called Flight of the Earls in 1607 the act of defeat and despair, or was it, as Micheline Kerney Walsh suggests in her study of the final fifteen years of Ó Neill's life, "a planned, tactical retreat" (p. 143)? Walsh's conclusions are based on a detailed study of newly discovered documents in Spanish and French archives, including correspondence by and about Ó Neill. The book begins with a preface by Cardinal Ó Fiach, which misleadingly implies that Walsh's work is in the tradition of Irish hagiography: "Ó Neill emerges with enhanced stature and more impressive gifts, a giant among his contemporaries and a leader whose place in our history has not yet been accurately appraised" (p. xii). The preface is followed by Walsh's 143–page analytical narrative of Ó Neill's life after his defeat in 1601–02 at Kinsale. This section includes translations of many lengthy excerpts and occasionally full letters. The second section, "Documents from Continental Archives," repeats these translations and includes other documents mentioned in passing in the narrative. The book concludes with a "Calendar of Documents" summarizing the letters in the previous section. Although each of these sections is valuable in itself, quoting or summarizing the major documents three times increases the length of the book and raises the price beyond the means of most scholars.

The picture of Ó Neill that emerges from Walsh's narrative is not of a man in the slough of despair but of a constant and usually optimistic leader, carefully weighing plans for his return from exile and for the overthrow of English rule in Ireland. Occasionally, a querulous note does enter Ó Neill's correspondence, especially when he is asking the Spanish to increase his allowance or even to answer his letters. If the Flight of the Earls was indeed a tactical retreat, it was based on the assumption that King Philip III of Spain had the resources and the strength of purpose of his predecessor. Time after time, Ó Neill and the other Irish exiles proposed plans involving a Spanish invasion of Ireland, but, Walsh argues, they were consistently frustrated by "the timorous attitude of King Phillip towards the King of England" (p. 100). Whether Philip and his ministers, especially his favorite the duke of Lerma, were merely being timorous, or whether they were correct that Spain's interests lay in retaining the friendship of James I, especially in regard to the Netherlands, may be less obvious than Walsh believes. Most of Walsh's conclusions, however, are commonsensical and based on prodigious research. Her work is an important contribution to knowledge of this period of Irish history.

DOUGLAS G. GREENE
Old Dominion University

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T. W. MOODY and W. E. VAUGHAN, editors. *A New History of Ireland.* Volume 4, *Eighteenth-Century Ireland, 1691–1800.* New York: Clarendon Press of Oxford University Press. 1986. Pp. lxiv, 849. $115.00.

In the twenty-five years since it was originally projected, the *New History of Ireland* has generated several ancillary volumes of reference material. Of the basic chronological sequence, this volume, edited by T. W. Moody and W. E. Vaughan, is only the second to appear. The delays, disputes, and deaths that have thwarted this enterprise raise questions not merely about the state of the historical profession in Ireland but also about the value of such massive collaborations in general. In a brief review, however, attention must be focused on the volume in hand.

It would be pleasant to record that this book, after its many vicissitudes (including the demise of a number of contributors), was worth waiting for. Unfortunately, one is struck by the feeling that much of its contents arrived a long time ago, in other wrappings. The main narrative chapters, spanning the years 1691–1800, were written by J. G. Simms, J. L. McCracken, and R. B. McDowell, all of whom published important studies during the 1950s and 1960s but do not seem to have developed any new sources or insights since then. The significant work done by younger historians during the past fifteen years, particularly in social and economic history, is virtually ignored in these rather musty and determinedly "political" chapters. To be sure, L. M. Cullen—perhaps the most original and creative contemporary interpreter of Irish history—has been assigned a chapter on "economic development," but he stands apart from the theme and tempo of the book. The volume also includes topical essays on literature in Irish and English, the visual arts, and music that are both comprehensive and useful but almost entirely lacking larger context.

Most disappointing of all is Simms's chapter on the Irish on the Continent. Well-known for his work on the Williamite Confiscation, Simms was not an appropriate choice for this assignment. The essay, which he apparently turned in five or six years before his death in 1979, is little more than a summary of miscellaneous information concerning the Irish abroad. It consists, in large part, of well-worn anecdotes about notable soldiers of fortune. Uninfluenced by recent scholarship in this field, Simms failed to grasp the complexity of the Irish diaspora, with its interwoven strands—military, mercantile, ecclesiastical—stretching across Europe, linking exile communities from Paris to Prague. He misses the importance of the relationship, both material and spiritual, between the Continental Irish and their compatriots at home. And he does not perceive the connection between this earlier emigrant experience and that of the nineteenth-century Irish. This most neglected dimension of eighteenth-century Irish history demands a more capable interpreter.

Eighteenth-century studies are alive and well in Ireland, as evidenced by the recent establishment of the Eighteenth-Century Ireland Society and the publication of its journal. The new generation of scholars, however, with its commitment to interdisciplinary approaches, is likely to find this volume sadly unsatisfactory. In its bulk and texture it reminds one of those megalithic structures scattered about the Irish countryside—massive, lonely relics of a world view that has ceased to be relevant.

WILLIAM D. GRIFFIN
St. John's University

LEAH LEVENSON and JERRY H. NATTERSTAD. *Hanna Sheehy-Skeffington: Irish Feminist.* (Irish Studies.) Syracuse: Syracuse University Press. 1986. Pp. x, 227. $22.50.

This is an interesting study of a hitherto-neglected figure in Irish history. Hanna and Francis Sheehy-Skeffington, who married in 1903, shared a vision of Ireland that joined elements of pacifism, antimilitarism, and a dislike of churchly authoritarianism and the excesses of nationalism to a deeply felt feminism. Although Francis was shot at the age of thirty-seven on Easter Wednesday 1916, Hanna lived until 1946 devoting herself to a variety of reform causes through five decades marked by political upheaval, repression, and violence. As a writer and editor, Hanna made vital contributions to the *Irish Citizen*, her husband's paper, perhaps the most important voice of the feminist movement during the early years of the twentieth century. She was a prime figure in the formation in 1908 of the Irish Women's Franchise League, the strongest force in Ireland for obtaining votes for women. Basically a shy woman, she became a powerful speaker for reform causes and even made lecture tours in the United States to bring the Irish revolutionary message across the ocean. In 1943, at age sixty-six, she stood for the Dail in the general election as an independent candidate for the Dublin South City District, an area she had represented from 1918 to 1925 in the Dublin Corporation. She pressed for such feminist issues as equal pay for equal work, equal opportunities for women, the removal of the marriage ban on teachers, doctors, and other skilled women, and proper pensions for the aged, widows, and the blind. Though defeated, Hanna was satisfied that her campaign had stirred positive public debate on those issues. After her

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death in 1946 messages of praise and tribute testified to Hanna's dedicated public career. To measure the significance of her work for a pacifist and socialist Ireland is not easy. Her lasting contribution toward obtaining the vote for Irish women is unquestioned.

Leah Levenson and Jerry H. Natterstad have collaborated on a well-written, carefully documented study. One must always applaud the appearance of a serious biography of a neglected historical figure. Hanna Sheehy-Skeffington was a major figure in Ireland during a turbulent period, and this study helps clarify our picture of her and place her feminism within the context of other Irish reform efforts.

SHEILA R. HERSTEIN
City College
City University of New York

FRANÇOIS FURET and JACQUES OZOUF. *Reading and Writing: Literacy in France from Calvin to Jules Ferry.* (Cambridge Studies in Oral and Literate Culture, number 5.) Paperback edition. New York: Cambridge University Press or Maison des Sciences de l'Homme, Paris. 1982. Pp. ix, 369.

This is a clear translation of the first volume of François Furet's and Jacques Ozouf's *Lire et écrire: L'alphabétisation des Français de Calvin à Jules Ferry*, published in 1977. The work is well known to historians of France, and the translation makes it available to a much broader range of historians of literacy and of education as well as to social historians generally. It is a superb example of Annales-school historiography and employs quantitative analysis intelligently. This edition might have included some addenda, such as reference to scholarship of the last decade concerning schooling in France and more comparisons with other countries, but the original work was outstanding and stands on its first merits. This brief review can mention only a few of its contributions.

The book begins with a careful reevaluation and verification of the authors' most important source: Maggiolo's compilation in 1877 of literacy records over two hundred years. Maggiolo was most interested in differences within France. Themselves historians of France rather than of literacy generally, Furet and Ozouf pursue that interest. They describe a clear line between St. Malo and Geneva that demarked the literate northeast from the rest of France during the *ancien régime*. During the nineteenth century other areas of France achieved universal literacy and schooling; a single area of retardation remained at the birth of the Third Republic—a triangle extending from the Atlantic into the west-center.

Emphasizing that literacy developed independently from French politics, the authors treat the history of literacy as an important topic in itself rather than as an aspect of political history. The revolution was not a decisive event in the history of literacy. Even the famous and bitterly debated Ferry Laws, which made school compulsory, free, and lay, gave official recognition to long-established trends. Social demand, not the state or the church, governed the development of literacy and schooling.

Male literacy preceded female literacy, but male literacy had more irregular patterns than did female literacy, which, having taken the first step, marched steadily forward. During the nineteenth century gaps between male and female literacy were gradually filled. Socioeconomic factors were far more determinant of the literacy of men than of women. The schooling of girls, in fact, was one area in which legislation spurred social trends.

Literacy in urban areas exceeded that in rural areas, but the authors go beyond that crude and overused index. Established cities engendered literacy; rapid industrialization or urbanization resulted in lower literacy. The social framework, particularly the existence of petite bourgeoisie or an affluent peasantry, was more important than urbanization per se; the appropriate social framework was more common in cities.

In their concluding chapter the authors offer a number of general conclusions about the social implications of literacy. Reading permitted one to receive information; writing permitted one to communicate. Writing, then, signaled the emergence of the individual out of the community. It weakened the power of the priest and the old, whose functions derived from the needs of an oral society. It also nourished the burgeoning power of the state, whose edicts were written. Ultimately, new communications at the personal level altered society and government and created a need for reintegration. The authors' explanation for the growth of literacy among different groups and areas and their conclusions about the effects of mass literacy on society could serve as paradigms for study by scholars of other countries.

PATRICK J. HARRIGAN
University of Waterloo

HENRY HELLER. *The Conquest of Poverty: The Calvinist Revolt in Sixteenth-Century France.* (Studies in Medieval and Reformation Thought, number 35.) Leiden: E. J. Brill. 1986. Pp. xiii, 281. f. 98.

This investigation of the early French Reformation presents readers with a rather odd and often unappetizing menu. The unwary should be warned that

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they are confronting a watered-down Marxist interpretation of French Calvinism before 1560, dressed out in ragged prose and hiding behind a title that obscures its contents, since Henry Heller's book rarely deals directly with poverty and stops before the Wars of Religion begin. Much of it consists of in-depth studies of dissent in various towns scattered across France. Some episodes work better than others; the overall effect is uneven, and the whole amounts to something less than the sum of its parts.

The coherence and cohesion of Heller's approach fall mainly between chapters 2 and 5. He sensitively explores the first, artisan-based congregation of French "Lutherans" at Meaux; he inspects the Protestantism of small-town notables at Agen; he devotes a section to Calvin's personal role in trying to unify such disparate movements before 1550; and he explains the successful fusion of artisans and notables at Troyes in the 1550s. The use of inquisitional sources—so valuable for our knowledge of Italian dissenters of the same period—informs his accounts of events at Meaux and especially at Agen; the diary of a Calvinist notable, Nicole Pithou, offers invaluable insights into the marriage of convenience between artisans and bourgeois in the 1550s. Perhaps because they lack such privileged sources, his other case studies (Rouen, Poitiers, Tours, Valence) neither advance his thesis very persuasively nor hold his reader's interest very well. By the late 1550s in France, as Heller says, "heretical belief seems inextricably mingled with fiscal grievance, economic anxiety and political exasperation" (p. 179). Intelligent aristocrats understood this and quickly moved to seize control of the Huguenot movement by 1561, with Calvin's enthusiastic support. Heller says little about this process (a story that has been told well enough by others) since only artisans and notables, the capitalist and proto-industrial elements of French society, concern him. Unfortunately, only the nobility of the sword could make Calvinism into a viable national church (or antichurch) in sixteenth-century France.

But the road to these conclusions is paved with clanking prose and cluttered with Marxist platitudes. The red pencil comes out to correct "hung" (pp. 16, 188) into "hanged" and "defiling" (pp. 44, 66) to "parading," to respell "payed agent" (p. 129) and "rifen" (p. 240), and to excise an uglification such as "evangelicize" (six entries). Proofreaders sleep while the "first and second estate" unite against the clergy at an estates-general (p. 245). As for the Marxism, savor this morsel: "While we suggest certain correlations between economics and religious change in . . . this study, we are far from claiming that they are either exact or exhaustive. We invite other scholars to join with us in trying to make them so" (p. x). An East German could not have said it better. Notwithstanding, a careful and patient reader can glean some useful insights about early French Calvinism here.

WILLIAM MONTER
Northwestern University

DANIEL HICKEY. *The Coming of French Absolutism: The Struggle for Tax Reform in the Province of Dauphiné, 1540–1640*. Buffalo, N.Y.: University of Toronto Press. 1986. Pp. 273. $35.00.

In this book Daniel Hickey uses an extensive analysis of the conflict surrounding the assessment and collection of the direct royal tax of the *taille* to explain social groupings in Dauphiné from 1540 to 1640 and their response to the encroachments of absolutism. He asks what made Dauphiné the first major province with an estates to lose its independent tax-granting assembly to the new absolutist state (p. 13). His starting point in answering is the *taille* controversy of 1579–1580, which culminated in the popular revolts described by L. Scott Van Doren and Emmanuel Le Roy Ladurie. Hickey agrees with Van Doren that the *taille* had escalated in the previous decade (pp. 45–46), but he disagrees with Le Roy Ladurie's "personalizing" the revolt in his book *Carnival in Romans* (1979), by attributing "an overwhelming role" to Jean de Bourg as a popular leader of the protest (p. 47). De Bourg was influential but not alone; rural resistance in the countryside was as important. From the 1530s to the 1570s rural villages instigated the protest against royal taxation, but in the 1590s their role was assumed by an urban elite (p. 70), the legal, merchant, and bourgeois groups who were most heavily taxed (p. 90). After 1602 the crown's intervention in the *taille* dispute began in earnest for fiscal reasons because the king needed money (pp. 166, 178). In 1628 the crown took the drastic measure of suppressing the estates and establishing royal tax officials. Popular protest followed, but the estates were not re-established in Dauphiné.

Hickey accepts the traditional explanation that social divisions caused by the *taille* dispute were responsible for the absence of organized opposition to the crown (pp. 174–75). The poor rural villages and nonnoble urban elite who composed the third estate concluded they had nothing to gain within the existing structure of the estates and supported the crown. They gained an immediate victory in the creation of a provincial assembly giving them greater proportional representation, but they lost in the long run because the assembly had only a consultative role (pp. 176–78). The Third Estate made common cause with the crown against "the Fourth Estate," to use Denis Richet's expression. Hickey agrees with Richet that new nobles from the urban elite, former military captains, nobles of the

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robe, and officials formed a separate social group that led provincial resistance to royal tax demands and whom the Third Estate deserted (pp. 138, 178, 189–90). The old established noble families were economically stagnant, but their dominance was not challenged by the new group of nobles. Hickey sees no signs of a sixteenth-century crisis of the nobility (pp. 154, 157, 160). The nobles were the principal source of resistance to the objectives of the Third Estate and the crown (pp. 159–60). Hickey accepts the thesis of René Pillorget that local conflicts among social elites were a basic element in popular revolts and a basic difficulty in presenting a unified opposition to the crown (pp. 8–9, 98). He rejects the interpretations of Boris Porchev, Roland Mousnier, and Alexandra Lublinskaia that there was a confrontation of social classes or orders and that the crown succeeded in establishing absolutism because it was supported by the bourgeoisie, however that group may be defined. He prefers the interpretations of Richet, Robert Mandrou, and Pierre Goubert, who emphasize the leading role of the urban elite and its relative independence from the lower classes and the first and second estates. This group resisted royal intervention and paid the price of the Third Estate's support of the crown (pp. 3–13, 68, 130, 174–78, 189–90).

A study of Dauphiné in the sixteenth and seventeenth centuries is much needed and long overdue. This book is an example of the excellent work that has been done in recent years by English-speaking historians of early modern France, work that is too often ignored by colleagues across the Atlantic. Surprisingly, Hickey does not mention some of this work, for example, the recent book by William Beik on the neighboring province of Languedoc in the seventeenth century, which is cited in the bibliography but not mentioned in the text, and the recent book by J. Russell Major on the Estates, which is mentioned only briefly in the text. Both Beik and Major have used the crown's collection of direct taxes to evaluate how absolute the monarchy was and refer to its use of the client system, as does Robert Harding. Hickey makes no mention of Harding's book on the provincial governors and their use of client networks in the late sixteenth and early seventeenth centuries. These books represent an "institutionalist" point of view on the origins of absolutism that Hickey does not share, but I am not sure this viewpoint can be ignored in discussing the demise of an institution. In general, Hickey has traveled a well-trodden path: he has carefully situated his case study of Dauphiné within the standard French historiography and has not offered novel explanations. Nonetheless, he has produced a carefully researched, lucidly written study of a province that has long been neglected. His book will be most useful for specialists in the field for this reason.

SHARON KETTERING
Montgomery College

FRANÇOIS BILLACOIS. *Le duel dans la société française des XVI^e^–XVII^e^ siècles: Essai de psychosociologie historique.* (Civilisations et sociétés, number 73.) Paris: Ecole des Hautes Etudes en Sciences Sociales. 1986. Pp. 539. 330 fr.

This is a very important study. Scholars of early modern France have been waiting for many years to see the results of François Billacois's researches on the duel. I think they will not be disappointed; I certainly was not. Billacois has chosen to consider the duel as a phenomenon in itself. Thus, instead of primarily using the duel to help understand absolutism, the nobility, noble attitudes, and the like, he uses the latter to help understand the duel, which remains his primary focus. This approach enables him to treat many important themes and to study just about any aspect of the duel conceivable yet remain within the framework of a certain organization, albeit a tenuous one.

The modern duel, as we know it, took form in France in the early and mid-sixteenth century, underwent important changes and modifications as it reached its height toward the end of the sixteenth and the first half of the seventeenth centuries in the form of the duel of the *point d'honneur*, and then gradually declined in importance—without disappearing entirely—in the period after the Fronde. Billacois analyzes its origins, arguing for general continuity between the judicial duel of the Middle Ages and the duel of the *point d'honneur*, which, he argues in an interesting comparative section, was far more important in France than in Germany, England, Spain, and Italy. The heart of his study concerns the duel of the *point d'honneur* at its height. Here, in analyzing the arguments for and against the duel, the points of view of the nobles and others, the significance of the famous execution of Bouteville, and much more, he carries out a remarkable study of collective mentalities and shows how important the belief in the duel and the values it represented were, not only—if primarily—for aristocrats but also for kings, nonnobles, and even church people. He then discusses the duel's decline and, at the end, abandons his chronological approach to examine the duel of all three periods in relation to psychological, anthropological, theological, literary, philosophical, and other themes.

The range of learning that went into the writing of this work is breathtaking. But, at times, a clear direction and answers to some of the basic questions

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are lost in all this erudition and information. Billacois himself admits, for instance (p. 336), to not being able to answer the question of why duels proliferated in the late sixteenth and first half of the seventeenth centuries. Billacois also draws innumerable conclusions here; some are carefully balanced and nuanced and based on an immense documentation, whereas others take one or two questionable references much too far. Billacois also tends to accept traditional definitions of "noble" and "bourgeois" (except for one reference to a work of 1960 by François Bluche [p. 326]), even though much recent scholarship is moving in different directions and his own material often suggests modifications. The book is never dull, however. It challenges constantly, does not waste space, and will teach an enormous amount to those willing to make the necessary effort.

ELLERY SCHALK
University of Texas, El Paso

D. M. G. SUTHERLAND. *France, 1789–1815: Revolution and Counterrevolution*. Reprint. New York: Oxford University Press. 1986. Pp. 493. Cloth $32.50, paper $12.95.

First published as the initial volume in a series on the history of modern France, the new book by D. M. G. Sutherland is one of the most important syntheses on the revolutionary and Napoleonic period to appear in many years. Like many contemporary students of the period, Sutherland discounts as untenable the Marxist interpretation of events—or, at least, the more exaggerated versions of that interpretation. But, unlike the majority of "revisionists," who despair at identifying any general patterns or who tend to reduce everything to politics, Sutherland continues to affirm the importance of social conflict as a central motor force in the revolution. He does not, to be sure, entirely neglect political forces: the collapse of royal authority and the consequent power vacuum in 1789, the dialectic of political factions within the national assemblies, and other such matters are given substantial treatments. But, in the author's view, it is a mistake to ignore the substantial antagonisms that existed in 1789 between the majority of the aristocracy and the majority of the middle class, antagonisms born of real and economically significant differences in wealth and privilege between the two groups, antagonisms that were sometimes, but often only superficially, overridden by the new values of the Enlightenment. But social conflict played a critical role in another respect: in the reaction of rural France to this revolution that was ultimately based and conceived in the city. Expanding on themes elaborated in an earlier book on the Chouans and in articles published with T. L. A. Le Goff, Sutherland stresses the "weight of ancient peasant France," which bore down so heavily on all the political regimes from the Constituent through the empire. The economic gain and loss of the country people at various stages of the revolution were often profoundly different from those experienced by the town dwellers; the revolution's impact on "ancient cultural norms" could be wholly unacceptable. The "counterrevolution" that developed among the rural inhabitants in certain regions of France, on the one hand, and among the aristocratic reactionaries, on the other, was integral to the revolutionary dynamic from beginning to end.

The text as a whole is characterized by its dogged insistence on re-examining a range of issues pertinent to revolutionary and Napoleonic studies. The nature of the Terror, dechristianization, the forces of popular religion, the Ventose Laws, the objectives of the Directory, and the continuities between the Napoleonic and the earlier revolutionary regimes are only a few of the topics subjected to critical reevaluations. Moreover, Sutherland devotes large sections to exploring the regional texture of events—the experience of the revolution in the provinces and the striking regional variation in the perception and reception of policies established in Paris. In the process the author succeeds in integrating a remarkable number of local—and frequently obscure—secondary studies. All of these are delineated in a valuable annotated bibliography.

The text is demanding and challenging. The author assumes considerable knowledge of French geography, institutional terminology, and revolutionary historiography. Compared to most other accounts, Sutherland's book provides little treatment of the background of the Old Regime and the Enlightenment. The book may well prove difficult for beginning undergraduates. Nevertheless, in my view, it is the best synthesis on revolutionary and Napoleonic France presently available. It will be required reading for any scholar interested in this period or in revolution in general.

TIMOTHY TACKETT
Catholic University of America

HUBERT C. JOHNSON. *The Midi in Revolution: A Study of Regional Political Diversity, 1789–1793*. Princeton: Princeton University Press. 1986. Pp. viii, 309. $50.00.

Insofar as a thesis can be excavated from this aggravating book, it is that moderate politicians in the south of France during the revolution were unable to maintain themselves in office in violent areas and were able to do so in calm ones. Plausible as this

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might appear to be on the surface, Hubert C. Johnson's deployment of the idea presents a number of problems. In the first place, he rarely defines his terms. The Midi is a much larger place than the eight departments he has inexplicably chosen for his study, and, bafflingly, he has excluded the Ardèche, a hotbed of popular counterrevolution. The author also never explains what he means by "moderate" and "radical." These terms are more complicated than one might think. Both became terms of abuse, both changed their meaning over time, and both meant different things to different people. To believe that the "federalists" of 1793 were moderates, for instance, is to be hoodwinked by Jacobin propaganda. In fact, the federalists in power behaved much the same way as the Jacobins did.

If the terms of reference are hazy, so too is much of the explanation. Thus, to assert that the counterrevolutionaries wished to restore the absolute monarchy is simply wrong. In fact, many counterrevolutionaries such as the Comte d'Antraigues wanted to reform the Old Regime as drastically as any Jacobin did. The research is incomplete as well. The author offers a number of figures and tables designed to illustrate regional variations in violence, yet none of this can be trusted since he has omitted a number of major series in the Archives nationales and neglected a couple of major printed sources. He does not show anything about the turnover of political personnel during the period. All that is presented is a too brief discussion of the social backgrounds of some municipal politicians in 1790. The thesis obviously requires more than this. He also misunderstands popular violence. To limit popular motives to the economic sphere is to neglect the role of sectarianism, fear, rumor, incitement, and sheer fun that also stimulated ordinary people. The author also makes major factual errors, as when, for example, he persists in claiming that passive citizens existed as a category after 1792. Finally, there are innumerable unsubstantiated remarks, a woefully incomplete bibliography of archival and primary sources and secondary studies, frequent passages of a meandering, allusive, and stodgy narrative, and an excessive dependence on secondary works of uneven value. In short, this book hardly does justice to the confusing, violent, breathtaking, and ultimately very sad revolution in the Midi.

D. M. G. SUTHERLAND
University of Maryland

FRANÇOIS DELAPORTE. *Disease and Civilization: The Cholera in Paris, 1832*. Foreword by PAUL RABINOW. Translated by ARTHUR GOLDHAMMER. Cambridge, Mass.: MIT Press. 1986. Pp. xiii, 250. $22.50.

This book was originally written in French, but this fine translation, with a useful foreword by Paul Rabinow, marks its first publication. François Delaporte offers us a "snapshot" of medical "practice" in France during the cholera epidemic of 1832, which took eighteen thousand victims in Paris alone. A student of Michel Foucault, Delaporte justifies this study of "practices" by asserting at the outset that disease "does not exist"; all we have are the "beliefs, cognitions, and perceptions" we use to understand disease and the "methods" we use to cope with it (p. 6). In advancing this methodological fiat Delaporte does not hope to sow epistemological confusion but to focus our attention on the myriad cultural, medical, and political reactions to the invasion of *cholera morbus*, about which there was little useful epidemiological or medical knowledge at the time.

When the ravages of the epidemic had ended, doctors and public hygiene officials had learned only slightly more about the incubation, etiology, and morbid pathology of the disease, but the huge technical literature they generated tells us a great deal about the rich web of cultural associations and convictions in which their medical outlook was embedded. Delaporte brilliantly exploits this literature to illuminate the debates between the principal schools of medical thinking of the era, their responses to the epidemic, and their influence on the public resources mobilized to combat the death and disorder spawned by the disease.

Among the most deeply rooted medical beliefs of the time was the conviction that the victims of the disease, who were overwhelmingly the impoverished inhabitants of the capital's most crowded quarters, were in some sense responsible for their own vulnerability. Whether one adhered to the contagionist theory that cholera was spread by contact or preferred the competing infection theory, which regarded insalubrious habitats as breeding grounds for the disease, all agreed that the vices, the violent political passions, and the relative barbarism of the poor weakened the resistance of their constitutions and marked them out for the reaper's scythe. In opposition to this set of bourgeois medical conceptions, the poor believed they were being systematically poisoned by their class enemies and attacked or shunned the medical authorities attempting to aid them.

In general, as Delaporte shows, medical theory and practice respected the social and political cleavages in Paris of the July Monarchy. The rich, through the public health apparatus they controlled, hoped to confine the disease to the poorer quarters, but they resisted the radical implications of the epidemiological theory they favored. They preferred cheaper traditional practices such as charity and treatment of the sick rather than the kind of

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root and branch urban renewal that the infection theory required to put an end to epidemics.

The most remarkable thing about this splendid book is that it will prove itself useful both to specialists in the history of science and medicine and to historical generalists. Delaporte's learned and exhaustive readings of the medical texts brim with subtle observations and new insights, and his treatment of the cultural and political implications of medical thinking should prove useful to all European historians of this period. In Delaporte's capable hands Foucault's methods come very close to fulfilling their considerable potential as history and criticism.

ROBERT A. NYE
University of Oklahoma

HARRY W. PAUL. *From Knowledge to Power: The Rise of the Science Empire in France, 1860–1939*. New York: Cambridge University Press. 1985. Pp. ix, 415. $49.50.

The subject of Harry W. Paul's interesting book is the institutional modernization and expansion of French science during the Third Republic. The main emphasis is on developments within the universities, but there are other motifs. The author has not intended a comprehensive account of the entire range either of French science or of the institutions that accommodated it. He makes only cursory mention of astronomy and geology and incidental mention of mathematics, physics, and chemistry together with passing remarks about the Bureau des longitudes, the Collège de France, the Muséum d'histoire naturelle, and the Institut Pasteur. Paul has rather chosen to write a series of essays on topics that illustrate the increase of scale and the response of government and other agencies to the necessity of underwriting a modern scientific establishment. The most extensive chapter concerns the transformation of the Napoleonic university, virtually moribund by the time of the Second Empire, into the vigorous, though still largely autonomous, faculties of the University of Paris and of half a dozen provincial universities on the eve of World War I. The pull came from the demand for higher education on the part of a lesser bourgeoisie caught up in a belated process of industrialization. The push came from the state acting through the agency of the Conseil supérieur de l'instruction publique animated by the anticlerical hand of Jules Ferry and inspired by intellectuals, notably Paul Bert and Emile Durkheim, who perceived science as the unifying basis for a rational republican ideology. All the while the teaching mission of the faculties was complemented by a new provision for research afforded by the Ecole pratique des hautes études, founded (it is true) in 1868 in response to the German challenge but nurtured by the Third Republic.

Of the eight remaining chapters, the most original are the discussions of science in agriculture and of the scientific curriculum developed in the Catholic universities. I had not known that the perennial campaign of the agronomists of the eighteenth century and the scientific reformers of the nineteenth, notably Jean-Antoine Chaptal followed by Justus von Liebig, came to fruition in France around the turn of the present century. Neither had I known anything of the counteroffensive in science mounted by the Catholic faculties founded independently of the state after 1875 nor of the extent of their contribution to French science since then.

Biology is the only science that Paul discusses extensively. This is not the place to develop the point, but I was not persuaded of the relevance or cogency of his attack on Comtean Positivism, to which he denies all value as a stimulus to French biological work. The topics of his other chapters, on the ties of the provincial universities to local industrial interests, on the proliferation and mode of scientific publication, and on the financing of French science in the twentieth century, have been touched on in other recent writings. Nevertheless, Paul's knowledge is extensive and exact, and he adds greatly to our information on these matters.

I wish Paul and other scholars would address one overarching problem. He writes about scientific institutions and their relation to culture and the state instead of the content of any of the sciences. Strikingly, in France and elsewhere those institutions have become more and more like each other, although their backgrounds in the several national institutional structures are different. Why is that? Have content and practice had a more powerful influence on them than context? In the present climate the thought is heretical, but, if the reverse were true, why is the pattern convergent instead of divergent? Did Bert and Durkheim have a point?

CHARLES C. GILLISPIE
Princeton University

WILLIAM SERMAN. *La Commune de Paris (1871)*. Paris: Fayard. 1986. Pp. 621. 145 fr.

Just as the French body politic in the 1980s seems to be entering an era in which ideological pulls are diminished, so too the study of the Paris Commune of 1871, one of the most divisive events in French history, appears to have reached a level at which ideological distortions are reduced or eliminated.

Scrutiny of the Commune has diminished since the great outpouring of 1971 to mark the centennial of that event. The market and academe were satu-

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rated, but, fifteen years later, the earlier works have been digested and historical writing is set to move on to an even more nuanced and sophisticated synthesis. William Serman's book is such a work.

Works on the Commune typically begin with its antecedents: almost one-third of this book is devoted to background material. Serman's ensuing account of the Commune displays the meticulous attention to detail and extensive quotations so characteristic of French scholarship. Chapters 11 and 12, in which the author discusses the power of the Commune and its management of government, are the core of the book. He evaluates the Commune by the standards of its professed ideals. For example, the Commune stood for direct democracy, meaning that its deliberations should be public and that elected officials were answerable to public opinion, were to act on an "imperative mandate" from the public, and should step down if the electors so demanded. On each of these points the Commune was found wanting. Although sympathetic to the Commune, Serman characterizes its government as functioning with a "tragic anarchy that limited its work to pinhole realizations and to illusory statements of principle" (p. 347).

Although practically no area of life, from the arts to teaching, was unaffected by the Commune, its social and economic policy was the touchstone of its character. Serman reveals that he is abreast of current scholarship in these areas, for he concludes that, despite the paucity of socialist measures, the Communards believed they were starting down the highroad to the extinction of capitalism by encouraging unions to form cooperatives in workshops abandoned by their owners.

Never have the ambiguities of the Commune stood in such sharp relief as they do with Serman's book. For example, the author points out that, at least theoretically, the Commune, as the municipal government of Paris, possessed complete legislative and executive powers, but it tried to assume the responsibilities of a national government, for which it had "neither the structure nor the authority" (p. 333). Would the Commune exercise power as the National Convention had in 1793–94, or would it be like its namesake of 1793 and try to influence power? Most in the Commune saw their government as a counterweight to existing government, which frequently was nothing more than the existing municipal administration over which Communards presumed to preside.

This is an account of the Commune seen with cold, unblinking eyes. Serman's work avoids the leftist romanticism found to a greater or lesser degree in such earlier standard accounts of the Commune as those in French by Georges Bourgin, Jean Bruhat *et al.*, and even the highly nuanced work by Jacques Rougerie or in English by Frank Jellinek. No aspect of the Commune escapes Serman's critical eye, although his criticism comes from a neutral, not a right-wing, perspective. Indeed, Serman's work is such a dispassionate treatment of the Commune that a reader unfamiliar with this event would be left wondering how the Commune inspired the thousands who fought and died for it or how it was such a symbolic event for the European Left. If the book has a flaw, it is that the events of the Commune have been carefully distilled so that the reader will not be led astray by leftist romanticism, but the price of having this refined product is that the reader cannot gain a sense of the emotion and dedication the Commune evoked. Serman concludes his book with the best treatment yet written on the "ideological transfigurations" of the Commune by both its proponents and its detractors.

Here we have a retilling of previous scholarship rather than freshly plowed ground, which rests on a broad base, as Serman's extensive bibliography reveals, but, regrettably, overlooks such insightful American works as Patrick Hutton's on the Blanquists and Bernard Moss's on French labor. The absence of footnotes is an annoyance for academic readers.

JULIAN ARCHER
Drake University

NANCY L. GREEN. *The Pletzl of Paris: Jewish Immigrant Workers in the Belle Epoque*. New York: Holmes and Meier. 1986. Pp. ix, 270. $39.95.

This book is a welcome contribution to a growing history of labor migration within idustrializing Europe as well as an uncommon treatment of East European Jewish settlement in France. Nancy L. Green's subject is the thirty-five thousand Jewish workers who mostly settled in the Marias district of Paris, known to them as the Pletzl. Rather than stressing assimilation—the cultural conflicts between immigrant and native or between assimilated Jews and their recently arrived coreligionists—she emphasized the economic conflicts between Jewish workers and their largely Jewish employers.

Green creatively employs push-and-pull analysis to explain the selective migration of Jews from Eastern Europe to Paris. Driven by both underemployment in skilled trades in the Pale and the Russian pogroms, Jews emigrated west. Those choosing to immigrate to Paris were attracted by both French cultural prestige and the demand for labor in light domestic industry, especially ready-to-wear women's clothing. She describes the "settling in" process as immigrant Jews met not only the ambiguity of indigenous coreligionists but also their own diverse cultural, especially religious, response to Paris.

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Green's real interest is the Jewish immigrants as workers and their conflicts with their Jewish employers. She carefully describes the domestic industries in which Jews predominated and the costs of "independence"—especially the ravages of seasonal employment and the "invisible fissure" that separated home from workshop. Strikes against Jewish employers, she argues, were not manifestations of differences between immigrant and assimilated Jew but a subset of class conflict peculiar to the competitive environment of the Parisian skilled trades in the generation before World War I. Jewish unionization proceeded apace with the growth of syndicalism in France. Although Jews mostly joined only ethnic sections of unions and the most successful example of immigrant unionism was the cap makers, in which Jewish immigrants were a majority, this was largely a function of language, native hostility to immigrant Jews, and the "specific exploitation" inherent in the domestic trades in which Jews predominated. Green offers the intriguing conclusion that the Jewish labor movement was a rejection not of ethnic identity but of the "paternalistic model of workplace solidarity encouraged by the immigrant milieu" (p. 190).

One wishes that she had developed this concept further. The reader never learns enough about Jewish employers or their paternalism. Although she stresses the roles of unions as instruments of economic struggle, her book offers rather better evidence that Jewish unionism was an expression of ethnic cultural and neighborhood identity.

Yet, as Green shows in rich detail, work and ethnicity could be mutually reinforcing identities. Her study offers fresh insights into this most complex problem.

GARY CROSS
Pennsylvania State University

GERMAN OJEDA. *Asturias en la industrializacion Española, 1833–1907*. (Historia.) Madrid: Siglo Veintiuno. 1985. Pp. xi, 472.

To what extent was a region endowed with coal and ores prone to industrialize in the nineteenth century? German Ojeda seems far too much inclined to assume that "where there is coal, there is everything" (p. 1). In his book the author gives a chronological account of the misfortunes of the northern district of Asturias in its drive toward economic growth. Two general questions are discussed. First, why did the Asturian regional economy grow so slowly, in particular during the first years of the period under scrutiny? And, second, why was Asturias subordinate to the neighboring Basque provinces, which were richly endowed in iron ores but totally deprived of coal?

Ojeda deals with the changes suffered by the coal, iron, and steel industries throughout the period 1833 to 1907. He has divided his work into three main sections. Part 1, "The Coal Years, 1833–1856," considers the first steps in coal mining and first attempts at developing a local iron industry. This section is an excellent example of the fundamental weakness of the book: its excessive reliance on the physical availability of coal with disregard for its quality, mining costs, and pricing policies. But, if Asturias failed in its attempt to become the Belgium of Southern Europe, the main reason has to be found, according to Ojeda, in lack of support from the state. Indeed, the author insistently argues that, by failing to undertake an expansion of the transport sector, the state prevented coal mining concerns from enjoying the advantages of a large market. Facing fierce competition from higher quality and cheaper British coal, Asturian coal was blocked near the pits. Therefore, Asturias might have lost its great opportunity to consolidate itself as Spain's main industrial district.

Part 2, "The Iron Years, 1856–1884," considers the period of Asturian hegemony over the Basque provinces. The comparative advantage Asturias enjoyed rested on coal and progress toward industrial concentration despite poor transport. Ojeda does not convincingly show that the short-lived success of the local ironworks owed much to the economic and technical rationale of the time, which advised that ironworks be located near coal pits.

Finally, in part 3, "The Steel Years, 1884–1907," Ojeda deals with the relative decline of Asturian steelworks in the face of growing competition from the Basque region once new technologies freed steel from its subordination to coal. On the other hand, reinforced protection and new developments within the transport sector at the turn of the century helped shift the Asturian economy toward specialization in coal mining. This final section is an interesting piece of work about the formation of the protectionist lobby—the building up of an alliance between coal and iron interests shared by Asturian and Basque manufacturers. As a result of their joint effort, protectionist bills were passed by parliament in 1891, 1896, and 1906.

Ojeda has performed scrupulous research in the archives of major mining and iron concerns. The reader will no doubt appreciate the formidable array of quantitative and qualitative sources. Reading might be slowed down, however, by the 113 tables inserted in the text, many of which are reprinted in the appendixes. Ojeda also excessively quotes the complaints of suppliers. Thus, by 1907 Asturias had become a net exporter of natural resources after frustrated attempts at becoming an important manufacturing region. One is tempted to think that the state had acted wisely for over half a

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century by not granting requested support to a noncompetitive and highly inefficient coal industry and that the support granted after 1890 was excessive. It is not clear why public money should have been invested in sectors in which private investors refrained from doing so. On the other hand, the Spanish economy might have been better off had Asturian coal not existed. After all, coal is not everything.

A. GÓMEZ-MENDOZA
Universidad Complutense, Madrid

BERNT DOUHAN. *Arbete, kapital och migration: Valloninvandringen till Sverige under 1600–talet* [Labor, Capital, and Migration: The Migration of Walloons to Sweden in the Seventeenth Century]. Summary in English. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 140.) Uppsala: Uppsala Universitet; distributed by Almqvist and Wiksell, Stockholm. 1985. Pp. 215. 130 KR.

The Swedish soil contains many minerals, iron and copper in particular, and ores have been refined there since the Middle Ages. By the seventeenth century both the crown and the diet sought to advance the quarrying and refining of iron, which was the country's most important export. Foreigners who brought necessary capital and professional skill made essential contributions to the rise of the Swedish iron industry.

In this thoroughly researched study Bernt Douhan analyzes the migration of Walloons to Sweden. He emphasizes their distribution among the iron works and their professional distribution. Douhan has compiled a file of the nearly one thousand Walloons who moved to Sweden and their offspring. Since records of the iron works have been well preserved and the series of Swedish source materials used for biographical study date back more than three hundred years, he has been able to collect a substantial file and to construct impressive tables. He concentrates on three main problems: the geographical settlement and movements of the immigrants, their professional distribution, and the character of their lives in Sweden.

Douhan's most important findings are the duration of the migration of the Walloons and the wide variety of trades they represented. Immigration began at the end of the sixteenth century and continued until the 1670s. Thus, it did not occur in a single wave but continued over several decades. Further, the Walloons, who are usually considered to have been blacksmiths, had dozens of different trades. Surprisingly, most of the immigrants were woodcutters and charcoal burners, that is, menial workers that were not scarce in Sweden.

Douhan's method has its restrictions. Readers will want to know exactly how the Walloons were divided among the iron works only if they know the local conditions and Swedish geography. A more general discussion of the Walloons' importance would have been desirable. Douhan shows that skilled immigrants constituted nearly the complete staff of one iron works, whereas their contribution in another was considerably less significant, but he does not say whether the outputs of these works differed. Calculation of the production figures per employee would have been useful.

Wages could have been expressed more accurately in thalers, marks, and öres. Douhan stratifies the Walloons by trade and compares their sets of values and wages, but he neglects to prove his assumptions. Certainly, an experienced blast furnace master was paid better than his young assistant, but were Swedes and Walloons paid equally, and to what extent did wages vary between iron works? In addition, the administration of justice and other interesting matters are dealt with too concisely in the last few pages of the book.

Like most conventional European historical research, Douhan's book overlooks women. He assumes men worked, while their wives looked after the home. The death of the husband threatened the social and economic position of his family. Often the family ended up in the care of a male relative, or the widow found a new husband to support her. On the other hand, the author mentions that both men and women were required to do statute labor. Did the iron works and the employer then expect the whole family to participate in the work? What kind of work did the women do? Douhan might also have considered women when comparing the elementary industrial community of the iron works with the lives of farmers and bourgeoisie. Both rural and urban law allowed a widow to practice her late husband's trade as an economically independent person. Were the industrial communities different from the agrarian and urban environments? Was a scrivener's or a blacksmith's skill so much tied to his person that a widow could not hire someone to do her husband's work, even for the few years of her son's minority?

ANNELI MÄKELÄ
Helsinki, Finland

BENGT SANDIN. *Hemmet, gatan, fabriken eller skolan: Folkundervisning och barnuppfostran i svenska städer 1600–1850.* [The Home, the Street, the Factory, or the School: Popular Education and Childcare in Swedish Cities, 1600–1850.] (Arkiv Avhandlingsserie, number 22.) Lund: Arkiv. 1986. Pp. 320. 130 KR.

In the book reviewed here Bengt Sandin explores some antecedents to Sweden's general school law of

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1842. Yet this is not merely a history of education or administration. Informed by Marxist sensibilities and U.S. revisionist historians, Sandin analyzes printed and manuscript records of central and local governmental bodies. From these he delineates the debates within the previous centuries' ruling circles regarding control of the people, the role of schools in that endeavor, and educational policy. He further explores the resulting institutions, concentrating on urban schools and using the ones in Stockholm as a case study.

In the seventeenth century, according to Sandin, the ruling classes discussed and implemented various ways of assessing, registering, and controlling popular beliefs and behavior. The rural clergy, for example, held catechetical examinations in their parishioners' homes and prompted heads of households to teach and control their children and servants. By 1650 similar policies and activities appeared in Stockholm. Vain efforts were made to organize one school for all classes, since the private tutoring of wealthy children could not be controlled. Elementary schools for only lower-class children—*fattigskolar*, or poor schools—were then set up and run by the state church.

From the beginning, Sandin suggests, these schools had three tasks: to occupy the children and keep them off the streets (a concern also expressed in the Sunday schools for apprentices and journeymen); to socialize their charges in the proper religious and social values; and to teach reading, writing, and arithmetic. He further suggests that this mandate was predicated on the notion that lower-class parents were not capable of raising or educating their children by themselves. Not until the general school law of 1842 did structural changes in the system take place.

The political atmosphere at Scandinavian universities is socialist, and sometimes this leads to gratuitous works on the misery of the poor. Yet at other times, as with Sandin's book, this orientation informs some perceptive analyses of society and relations between its various classes. As Swedish society slowly moved from an estate to a class orientation, he suggests, the upper classes increasingly withdrew from the public life of street and square. At the same time schools as institutions of social control gained in importance, as he shows in this detailed, in places even pedantic, yet readable book.

CHRISTIAN D. NÖKKENTVED
University of Illinois, Chicago

MICHAEL ROBERTS. *The Age of Liberty: Sweden, 1719–1772*. New York: Cambridge University Press. 1986. Pp. ix, 233. $39.50.

Sweden's "Age of Liberty" began and ended with peaceful but, to Michael Roberts, real revolutions, which in 1719–20 overturned Caroline absolutism and in 1772 restored strong monarchy under Gustav III. During this period Sweden adjusted—albeit painfully—to small-power status under a constitution that gave the Estates virtually unlimited authority.

Robert's book is a commentary—rather than a narrative survey—on the beginnings of the regime, foreign relations, the workings of the constitution, the evolution of political parties, and the conflicts leading to the end of the era. Indeed, it is an important "insider's" contribution to a central controversy of Swedish historiography.

Since Gustav III's time, the rule of the Estates has traditionally been condemned for its weakness, ineptitude, corruption, and subservience to foreign powers. By the twentieth century, however, liberal historians offered a reappraisal, representing it as a promising experiment in parliamentarianism, tragically cut short.

Roberts, as in earlier studies, here persuasively reconfirms the traditional view. As tests of a liberal constitution, he asks "does it work well?" and "does it secure those liberties of the subject which it must be presumed to have been designed to preserve" (p. 91)? Ultimately, he holds, the constitution of 1720 failed on both counts. Legislation and administration became hopelessly complex and ponderous; politics were poisoned by "suspicion of the executive, constitutional hair splitting, the selfishness of vested interests, foreign corruption . . . the rage of party" (p. 110). Conflict between parties led all too frequently to irregular procedures and savage political persecutions.

By 1772 class conflict overshadowed party strife. Gustav III's coup that year thus carried through a badly needed political revolution and staved off—until his coup of 1789—an impending social revolution.

Yet the age left constructive legacies: constitutionalism, the most advanced parliamentary machinery and procedure anywhere by 1772, a politically active and socially conscious middle class, a law code that is still basically in effect. The economy progressed, and Sweden became, for the first time, a contributor to, as well as beneficiary of, cosmopolitan European culture. Above all, it left an ideal of "government by consent," which "gave a moral force to successive oppositions, until a true democracy was attained in the twentieth century" (p. 217).

Roberts thus sums up an epoch that has deeply absorbed him for over thirty years. More than this, his work now covers, in unbroken sequence and with impressive erudition, Sweden's history from 1523 to 1772.

H. ARNOLD BARTON
Southern Illinois University

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CARL G. GUSTAVSON. *The Small Giant: Sweden Enters the Industrial Era.* Athens: Ohio University Press. 1986. Pp. xi, 364. $23.95.

I believe that anyone interested in Swedish history—economic or otherwise—will find this book to be great fun to read. I certainly did.

Carl G. Gustavson describes the backgrounds, personal foibles, and economic contributions of virtually all the inventors and entrepreneurs who made major, or even not-so-major, contributions to Swedish industrialization before 1914. Some attention is also devoted to financiers and bankers. The lower orders, however, receive no attention. The underlying research is based overwhelmingly, if not exclusively, on secondary studies, which seem to have been thoroughly reviewed. The style of the book is chatty, not to say gossipy. Apocryphal stories are not excluded, although the reader should have little difficulty separating them from the hard facts.

If the book is intended simply as informative, light reading for historians, it is an unqualified success. That the book has been published by a university press and distributed to leading scholarly journals for review, however, indicates that it has scholarly and educational ambitions. Judged from that perspective, the book is less impressive. Thus, although I in no way regret having read it and I learned a good deal about some interesting and important personalities, the book did little to deepen my understanding of Swedish economic history. Despite the subtitle, the book neither describes nor tries to explain Sweden's entry into the "industrial era." A few summary comments on the general course of Swedish industrialization and what lay behind it, all based on the work of others, are not enough to provide substantial insight into the process of Swedish industrialization. Tree after tree after tree is described, but no map of the forest is provided. The reader looking for information about and explanations of Sweden's remarkable economic performance between circa 1850 and 1914 had best look elsewhere. The likely places remain the works of Eli Heckscher, Lennart Jorberg, Arthur Montgomery, and Torsten Gardlund.

Gustavson's concentration on individuals and their contributions would seem to place him in the entrepreneurial school of economic historians. Is he, perhaps, a disciple of Joseph Schumpeter? Although that may indeed be the case, it is not apparent from the contents of the book. Schumpeter is nowhere mentioned or cited. What is more important, although the author seems to believe that focusing on "flesh and blood persons" (p. x) makes Swedish economic history more real, he never transcends the anecdotal level. The interesting intellectual questions involved are never raised. How do the entrepreneurial efforts of a group of individuals constitute an industrial spurt? How do the level of demand, the legal institutions (prominently including the structure of property rights), the policies of the government, and other similar variables affect the behavior of entrepreneurs? How do cultural values and social structures affect the supply of entrepreneurs? Although the book provides some data to help others deal with these crucial questions, in itself it does little or nothing to answer them.

LARS G. SANDBERG
Ohio State University

R. W. SCRIBNER. *The German Reformation.* (Studies in European History.) Atlantic Highlands, N.J.: Humanities Press International. 1986. Pp. x, 78.

This ambitious, slender pamphlet is the first treatment in English of the German Reformation (and, for that matter, of the Reformation in general) from the perspective of social history—which is, as in other fields of specialization, increasingly in vogue in Reformation studies. As such, R. W. Scribner's pamphlet is eminently valuable and useful (not to mention readable) in that it summarizes the German Reformation in quite a different way than have previous surveys. Although Scribner relates a loose chronological story, he emphasizes thematic focuses, as the several chapter headings denote—"Social Location of the Reformation," "Politics and the Reformation," "Varieties of Reformation."

Scribner's book is, in effect, an extended annotated bibliography of recent major works on the Reformation. Thereby, it serves as a vivid reminder of the dynamic state of current Reformation scholarship, social historians deserving the credit for having provided the stimulus. The emerging view of the Reformation accepts the genuine religious point of departure in Luther's ideas, but goes on to argue that the movement of reform "quickly outran Luther's ideas and achieved a near-revolutionary impetus of its own" (p. 24). The story of the Reformation cannot be told without emphasizing the deep structural crisis in which German society (in contrast to other European countries) found itself. The Reformation was successful, that is, it became a major historical movement, because religion became the handmaiden for the attempted resolution of social problems—not in the sense that authentic religious ideas transformed society, but rather, that religion was used by social groups and classes to further their respective social agendas. Had they not done so, the movement of reform would have remained as restricted as had earlier movements.

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Scribner states the case well, if a bit prematurely regarding the validity of the monographs he cites.

HANS J. HILLERBRAND
Southern Methodist University

R. EMMET MCLAUGHLIN. *Caspar Schwenckfeld, Reluctant Radical: His Life to 1540*. (Yale Historical Publication, Miscellany, number 134.) New Haven: Yale University Press. 1986. Pp. xii, 250. $25.00.

R. Emmet McLaughlin's study on a sixteenth-century champion of religious tolerance stands in a good tradition of North American scholarship. Did not the late Roland Bainton begin his career with a major study of David Joris? If the present volume is any indication of things to come, Bainton may have found a worthy successor. Certainly, after McLaughlin's work Schwenckfeld's theology will have to be treated in the context of historical biography.

The first part of the book covers Schwenckfeld's Silesian career as courtier, reformer, bishop on his estate, and chief teacher and heretic of the Reformation party at Liegnitz. McLaughlin underlines the significance of Schwenckfeld's training as courtier in the service of the secular princes of Silesia. The carefully groomed image of the gentlemanly courtier charmed his contemporaries. McLaughlin believes that Schwenckfeld remained typically Lutheran until 1525. He explains earlier differences in emphasis through Schwenckfeld's lack of theological schooling and the anticlerical atmosphere of Silesia. Real disagreements with Luther surfaced only with the controversy over the eucharist. The author attributes the "definitive" break to the encounter with dreams and visions that "jolted" Schwenckfeld's circle in early 1525. Key features of this interpretation appear to follow the contours outlined earlier by Emmanuel Hirsch. New are the connections drawn between the spiritualist emphasis and Schwenckfeld's advocacy of religious tolerance. Schwenckfeld perceived a relationship between an "undue reliance on the written word and religious persecution." The resulting quarrels with the Lutherans left him politically isolated, marked him as a heretic, and resulted in his exile.

The second part of the book traces Schwenckfeld's sojourn in southern Germany from 1529 to 1540. The chapters are appropriately titled "Champion of Christian Freedom," "Knight Errant of Discord," and "Minister of the Glorified Christ." McLaughlin illustrates that, in controversy with the magisterial reformers and driven by the logic of spiritualism, Schwenckfeld shifted the focus of his religious concerns from the church to the home. He notes that women played prominent roles among Schwenckfeld's followers and speculates that his advocacy of a privatized religiosity without sacraments offered these admirers a sense of liberation from the tutelage of the professional clergy.

Of special interest is McLaughlin's discussion of Christology, the "capstone" of Schwenckfeld's theological development, which provided him with a sense of mission and became the distinguishing belief of his followers. Better than anyone before him, McLaughlin traces the development of Schwenckfeld's Christology from eucharistic concerns to its full doctrinal expression through his controversies with the magisterial reformers and other radicals. Schwenckfeld gradually extended the noncreaturely status from the post-Ascension Christ back to his conception and earthly life. In its mature expression Schwenckfeld's Christology postulated that God had been father to both the divine and human nature united in Christ's person. Christ had taken human flesh from Mary's especially purified flesh, but he had never been a creature. The book closes with the condemnation of this peculiar Christology at Schmalkald in 1540. Two maps locate pockets of Schwenckfeld's followers in Silesia and southern Germany.

By way of criticism, the treatise at times reads too much like a confutation of Horst Weigelt's *The Schwenckfelders in Silesia* (English edition, 1985). Weigelt traced Schwenckfeld's early moralistic concerns to mystical or humanist roots and believed that Schwenckfeld had become critical of Luther's views on justification by 1524. In contrast, McLaughlin emphasizes Schwenckfeld's Lutheranism into 1525. Weigelt saw an initial Zwinglian influence on the eucharistic position of the Silesian but considered the charismatic manifestations of 1527 to be less significant. McLaughlin stresses the indigenous nature of the eucharistic concerns and detects a crucial turning toward spiritualism in early 1527. Differences extend to Valentin Crautwald's influence on Schwenckfeld and to the development of Schwenckfeld's Christology. Weigelt believed that the main assumptions of Schwenckfeld's Christology were implicit in the writings of Crautwald and Schwenckfeld by 1528. McLaughlin gives a greater role to the controversies of the mid-1530s as shaping Schwenckfeld's final position. McLaughlin sometimes appears almost apologetic for Schwenckfeld's orthodoxy, although his point that the major reformers were not strictly orthodox according to fifth-century standards is well taken. Schwenckfeld's opponents are not given the same sympathy. Martin Bucer is described as "a devious, none too scrupulous manipulator of men and ideas, who threatened the weak and fawned on the powerful" (p. 129). Controversial, as well, is the claim that the Strasbourg Synod of 1533 and the subsequent decrees of 1534–35 sealed the "identification" of

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preachers with council. Finally, a conclusion would have given the book a more finished appearance.

These criticisms should not detract from McLaughlin's achievement. He has produced the richest and best historical biography of Schwenckfeld to 1540. In constant debate with previous scholarship, he advances our understanding of Schwenckfeld's intellectual evolution and enriches our appreciation of the complexity of the Reformation as a whole. Apart from a few printing errors (for example, the omission of note 50 on page 280), the volume makes excellent reading. It is a very welcome contribution to our understanding of this reluctant radical, whose descendents sought refuge on North American shores.

WERNER O. PACKULL
Conrad Grebel College
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RICHARD VAN DÜLMEN. *Die Gesellschaft der Aufklärer: Zur bürgerlichen Emanzipation und aufklärerischen Kultur in Deutschland*. Frankfurt: Fischer Taschenbuch. 1986. Pp. 204. DM 12.80.

This slim but important volume surveys the establishment and development of various "enlightened" societies, associations, and clubs in eighteenth-century Germany and analyzes their differing (and evolving) social composition, goals, and relationship to public institutions. These societies, taken together, were perhaps the most important means by which the German middle classes (*Mittelstand*) gained the group self-consciousness and self-confidence that underlay their gradual liberation from the sociopolitical constraints of the traditional "feudal" order. This liberation, Richard van Dülmen asserts, was accomplished by means of education (*Bildung*), but education in the sense of a purely individual self-cultivation cannot become a social force. For that to happen, it must be aggregated. The organizations dealt with in this book, as new forms of communication between heretofore isolated and separated elements of German society, including not only the various middle-class occupational groups but also nobles, officials, clerics, and so on, were precisely the means by which such aggregation occurred. Once in being, however, aggregation not only stimulated the elaboration of the educational process but also, in socializing that process, led to a gradual reformulation of the nature and goals of education itself. From this, in the course of the century, emerged a bourgeois elite that was self-confident not only culturally but also socially and politically.

The author distinguishes three distinct phases in the evolution of "enlightened" societies. Each phase (corresponding roughly to categories of early, mid-, and late Enlightenment) was earmarked by the emergence of new societies—which did not, however, replace earlier ones—with different social compositions, socializing rituals, agendas, and attitudes toward state authority. To oversimplify somewhat, each new phase was characterized by a widening social composition (with a pronounced tendency toward increased bourgeoisification), more "democratized" social interaction within the societies, an increasing orientation of agendas toward reform, including political reform, of institutions and practices in the real world, and a tendency toward greater independence from, and even hostility to, existing political authority. From the formal learned academies and purely literary societies of the early eighteenth century, through the "patriotic societies" and Freemasonry of midcentury, to the Illuminati and radical *Volksgesellschaften* of the 1790s—all of which are catalogued in an appendix—the author provides a solid, fascinating, and comprehensive survey of an important aspect of the "emancipation" of the German bourgeoisie, which has recently been the subject of much discussion and research.

JOHN G. GAGLIARDO
Boston University

ARLIE J. HOOVER. *The Gospel of Nationalism: German Patriotic Preaching from Napoleon to Versailles*. Stuttgart: Franz Steiner. 1986. Pp. 164. DM 38.

Since 1945 German Protestants have harshly judged traditional German nationalism. But no comprehensive history of religious nationalism in the Protestant *Landeskirchen* has yet appeared. Arlie J. Hoover's excellent book may be regarded as a contribution to such a study. He has dissected thousands of Protestant sermons preached during the War of Liberation, the Franco-Prussian War, and World War I.

According to Hoover, the rationalism of the Enlightenment, pietism's antipathy to dogmatism, and the epistemology of Kant converged by the time of the War of Liberation to undermine the dogmatic content of Lutheranism. Reacting to the humiliation of Napoleon's domination, pastors filled this void in Protestantism by conceiving of a romantic concept of the German folk. Following Koppel Pinson, Hoover argues that pastors endowed the folk with pietism's emotionalism and religious conceptions. Thus, they could conceive of the folk as a divine creation led on a special mission by divine providence.

Hoover may well have overstated a good case. Pietism was not entirely devoid of dogmatic content, and pietism allied with an aroused Lutheranism to dominate much of church life (but not academic theology) for much of the century. The question is

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whether he need hypothesize a dogmatic void to hypothesize a transferral of religious categories to a conception of nationalism. Moreover, pietistic conservative Lutherans suspected folkish religious nationalism for its political implications even after the era of German unification, and one must ask whether religious nationalism actually took the clergy by storm.

Much of the book analyzes the themes of religious nationalism. Pastors claimed that God favored the German folk in these wars. The author illustrates how pastors judged the cause of the folk in religious categories, such as by sensing in patriotism the infusion of the Holy Spirit. But why did even clergymen who subscribed to Reformation theology fail to sense a contaminating, competing religion in religious nationalism?

By Hoover's account, the content of religious nationalism had grown more secular by World War I. But Walter Bredendiek (in *Irrwege und Warnlichter*, [1966]) saw a far greater change by then, proposing that many pastors regarded war as a positive good. To what extent did religious nationalism change qualitatively from war to war? Moreover, wartime may have called forth an extreme religious nationalism, whose acceptance only grew in stages throughout the century. Whether a focus on wartime sermons obscures a more general development of religious nationalism is a further question.

Hoover's trenchant analysis of sermons is similar to results published by German scholars. But only his book compares these sermons in three wars over a century and, therefore, can show a high degree of continuity. Bredendiek's book as well as the following recent collections of valuable essays should be added to the bibliography: *Volk—Nation—Vaterland*, edited by Horst Zillessen (1970) and *Kirche zwischen Krieg und Frieden*, edited by Wolfgang Huber and Johannes Schwerdtfeger (1976).

DANIEL R. BORG
Clark University

HEINZ HÜRTEN. *Kurze Geschichte des deutschen Katholizismus 1800–1960*. Mainz: Matthias Grünewald. 1986. Pp. 280.

The title of this book may be a little misleading, especially to American readers, since its subject is not the Catholic church as such but rather the structure and activity of Catholic associations, what is known in Germany as *Vereins-* or *Verbandskatholizismus*. More an interpretive essay than a comprehensive history, the work is organized around two main themes: the associations' influence on society and politics, especially on the conflict between church and state, and the relationship between lay and clerical elements in the groups' functions, leadership, and internal organization.

Heinz Hürten begins his account in the early nineteenth century, when the destruction of the Old Regime in the wake of the French revolution had disrupted the organization of the Catholic church in Germany and left it unable to defend its institutional autonomy against hostile secular states. With a nonexistent or politically paralyzed ecclesiastical leadership, a small group of lay activists had to find ways of mobilizing mass support in defense of the church's interests. Hürten discusses the early forms of Catholic organizations, their intellectual justification, and their further development: the successful integration of clergy and episcopate into their activities, their response to political events and to the emergence of the social question in Germany, all culminating in the great, successful struggle against state intervention during the Kulturkampf.

Most of this will be familiar to readers of Karl Buchheim's *Ultramontanismus und Demokratie* (1963), but, in the chapters on the period after 1890, Hürten emphasizes his other main theme, explaining how Catholic organizations successfully warded off efforts at tightening episcopal control before 1914. After World War I, however, the author sees a growing tendency toward clerical leadership and a more church-centered organization embodied intellectually in the liturgical movement of the 1920s and organizationally in the Vatican-sponsored Catholic Action. This theme is also important in his discussion of Catholic organizations during the Nazi era, besides the more obvious topic of state persecution. Hürten's brief final section on the post-1945 period is centered around a discussion of the gradual dissolution of the world of autonomous Catholic associations and their incorporation into church organization (*Verkirchlichung*), which the author believes has substantially weakened Catholic influence on state and society in the Federal Republic.

Even given the work's character as an interpretive essay, I have to wonder if the author's main themes are adequate for his purposes. Focusing exclusively on conflict between church and state ignores the many instances of cooperation, which are important for understanding the nineteenth-century Catholic movement. The virtual lack of any detailed consideration of social and economic factors hinders understanding of the activities of Catholic trade unions and peasant associations or clerical attitudes toward them; long-term tendencies toward secularization in twentieth-century German society or the growing importance of the mass media in influencing public life may have had as much to do with the declining influence of Catholic associations as have organizational changes. In sum, I found the author's interpretation concentrated too much on the organizations themselves and too little on the social and

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political world they sought, often successfully, to influence.

JONATHAN SPERBER
University of Missouri, Columbia

DAN SIMON. *Das Frankreichbild der deutschen Arbeiterbewegung 1859–1865*. (Schriftenreihe des Instituts für deutsche Geschichte Universität Tel Aviv, number 6.) Gerlingen: Bleicher. 1984. Pp. 334.

The economist Max Wirth, engaged with his brother Franz in creating a national businessmen's newspaper, was also stirred by international events of the years 1859 and after. He wrote of the "criminal conduct" of the French in opposing German unity. He found that nation "decadent" and "illiterate," its finest sons having perished as Germans in the St. Bartholomew's Day Massacre. "The basis of his judgment of events was his solid belief in the superiority of the German race over the Romanic," relates Dan Simon (p. 38). Wirth believed that a brilliant future lay before humanity as soon as it came under German sway but that the French were likely to languish in the grip of hundreds of thousands of Fourierists.

Less frantic than Wirth, but still recognizing France as the enemy, was Leopold Sonnemann, then involved in founding the *Frankfurter Zeitung*. His great phobia was a French attack on the Rhine. Less well known, but more fanatical than any other of Simon's chosen was Nikolaus Hadermann, who denounced the French in his own small paper as a "gang of robbers," dreaming day and night of a Rhenish boundary. He maintained that the Gallic neighbors, a people without culture, had only sent troops to Italy to damage Germany.

In Simon's view Ferdinand Lassalle was more rational than the others. He respected French culture, supported Italian independence despite its French suppor., and did not expect that the armies of Louis Napoleon would reach the Rhine before daybreak. But Simon does not concentrate on Lassalle, and he moves on to white raven Moses Hess. Hess had working-class credentials, but he was a Francophile, even an admirer of Louis Napoleon. He made a flier to Cologne to help Lassalle, then quickly returned to more congenial Paris. Had he swallowed too much hostility toward the land he felt should lead the world?

Simon's evidence of German hatred for the Gallic foe, prevalent and virulent already in the 1860s, is the most impressive side of his book. That hatred, he demonstrates, was shared by men of intelligence and position. But Simon beclouds the matter by insisting that these attitudes have something to do with the working class. Karl Marx, Friedrich Engels, August Bebel, and Wilhelm Liebknecht devoted decades of their lives to working-class politics, but their opinions of things French are not found here. Even the unreliable J. B. Schweitzer spent more years in proletarian meetings than did Lassalle but is not quoted. Why are these men neglected when the nonentity Hadermann is given thirty pages? Even worse, there is nothing from workers themselves. Simon asserts, near the beginning, that their ideas on France would be difficult to uncover. No doubt they would, especially given the predilection of laboring people to leave foreign affairs to their betters. But the task would not have been totally impossible, and the results might well have been illuminating.

RICHARD W. REICHARD
Chicago, Illinois

NICOLAS TEEUWISSE. *Vom Salon zur Secession: Berliner Kunstleben zwischen Tradition und Aufbruch zur Moderne 1871–1900*. (Jahresgabe des Deutschen Vereins für Kunstwissenschaft.) Berlin: Deutscher Verein für Kunstwissenschaft. 1986. Pp. 333.

For approximately the past two decades, art historians both in the United States and abroad have attempted to "read" the texts of art and architecture—paintings, graphics, sculpture, and buildings—and to put these "texts" in historical, political, cultural, and social context. This effort can be seen with greater frequency. For example, major museum catalogues and monographs on individual artists or aesthetic movements invariably contain historical material as part of their volumes. The *Journal of Interdisciplinary History* and the *Art Journal* have recently devoted issues to the theme of the intersection of art and history. Nicolas Teeuwisse's study belongs to the growing genre of "art in context" monographs. As such, it provides an amply documented and intelligent summary of imperial Berlin's artistic culture around the turn of the century.

Teeuwisse organizes his work chronologically. He outlines the main features of Berlin's art scene decade by decade, beginning in the 1870s after German unification and ending with the turn of the century. The major focus of his discussion is the growing antagonism between conservative academic painters of official Berlin, such as Anton von Werner, and the "modernists," such as Max Liebermann, whose works received increasing publicity and attention from museum officials and individual collectors. Werner was not only one of the most important "court painters" associated with Emperor William II but also head of the Royal School of Fine Arts and the influential Artists' Association (Künstlergenossenschaft). Liebermann, in addition

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to being Germany's leading impressionist painter, helped found and was elected the first president of the Berlin Secession, an elite artists' organization devoted to the display of German and international modern art. These two painters, representing conservatism and modernism, served as foils for important aesthetic, cultural, and even political questions in turn-of-the-century imperial Germany.

A number of scholars have previously presented the dramatic confrontation between the old and new (notably in the series edited by Ekkehard Mai and Stephan Waetzold, *Kunst, Kultur und Politik im deutschen Kaiserreich*, Teeuwisse in his essay for a museum catalogue on Liebermann, 1979–80, Werner Doede in his lavishly illustrated *Die Berliner Secession* [1977], and Peter Paret's monograph on the same subject). Teeuwisse's monograph is more ecumenical in its subject matter—detailing the activities of Berlin's academic painters, art dealerships, museums, and art associations—and discusses the battles fought between conservatives and progressives over the direction German art should take. But, given the complex and changing structure of Berlin's late nineteenth-century art world, Teeuwisse was forced to sacrifice depth for schematic coverage. For example, the author mentions, yet hardly indicates the importance of, the pivotal retrospective exhibition of nineteenth-century German art at the Nationalgalerie in 1906 and the role it played in subsequent interpretations of German painting. Aside from this minor reservation, Teeuwisse succeeds in highlighting the crucial episodes in Berlin's transformation from a "barracks town" to a true artistic metropolis. Teeuwisse is to be congratulated for using a wealth of archival and secondary material to good advantage. This material illuminates the aesthetic contributions of the major German artists residing in turn-of-the-century Berlin and situates their works in their cultural and historical context.

MARION F. DESHMUKH
George Mason University

HELMUT AHRENS. *Das Leben des Romanautors, Dichters und Journalisten Theodor Fontane*. Düsseldorf: Droste. 1985. Pp. 439.

The title of this book, had its author preferred a more archaic mode, might well have been "The Life of the Apothecary, Poet, Novelist, Historian, Press Attaché, Journalist, Academy Secretary, Theater Critic, etc. Theodore Fontane." Helmut Ahrens, who has previously published popular biographies of Johann Nestroy and Ludwig Thoma, certainly covers all of these aspects of Fontane's life well and entertainingly. His book, as he generously acknowledges, owes much to the recent work of Helmuth Nürnberger and Walter Seidel, but even more to Fontane's own writings, and Ahrens is wise enough to let Fontane himself do much of the talking. Thus, we hear Fontane's own words on the money worries that constantly plagued him. His two illegitimate children, he wrote early, "are eating the hair off my head before the world even knows that I have any" (p. 112). and, despite his growing reputation, he found no true financial security until he was well into his fifties. Throughout, Fontane kept his good humor and his good sense. They show up, for instance, in his observations about history that Ahrens quotes and that seem as fresh and relevant as ever.

His main interest was always in *petit histoire*. Large events, he wrote, would never lack for chroniclers; he was after the "middling, even small occurrences," which he found both more intriguing and more revealing. His *Wanderungen durch die Mark* overflowed with them, of course, and his love of detail and of a good anecdote still makes this book as much of a joy to read as his novels. Besides, he was a skillful practitioner of oral history before that term had even been invented. Ahrens cites one memorable episode in which Fontane asked his sister to find out what she could about the murder of a Frenchman in the village of Dreetz in Ruppin County, which, according to the documents, had occurred in 1806. "It does not have to be true," he wrote her, "*vox populi* has the right to lie as much as it wants. It is called a 'myth' in that case and is treated with the highest respect by scholars and oddballs like me. So please drum up a few people in Dreetz: perhaps some shepherd's helper of descending mental development, some 'wise woman,' some soothsaying imbecile. Blast it all, one of them should have enough imagination to discover why the poor Frenchman got himself killed. I myself can and may not invent anything, for one thing because it is against my 'historical conscience,' and for another because I am really convinced that some upstanding citizen of Dreetz in truth killed the Frenchman the way one drives a stick into the ground, or for even less reason" (p. 200).

Fontane's little history, of course—in his journalism, his travel books, his novels—has given us truths that have escaped writers with far greater academic credentials than his apothecary's diploma. This has been particularly true, as Ahrens once again reminds us, of his observations on Prussia. As much as anyone else (including Heinrich von Kleist, whose Prussia really was a dream Prussia) he was the chronicler of its positive sides, beginning with his ballads celebrating Prussia's heroes, and ending with the figure of Dubslav von Stechlin. "He had the . . . self-confidence of those who had been there before the Hohenzollern, but it was a very quiet self-confidence, and if he ever did express it it was in the

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form of humor and self-irony, since it was his nature to provide everything with a question mark. His finest feature was a heart-felt humanity, and about the only things capable of upsetting him (who otherwise would humor anyone who thought that five was an even number) were pride and arrogance" (p. 391). Yet Fontane could also be vocal indeed about Prussian weaknesses, whether it was the signs of arrogance that Stechlin so detested or its shortage of imagination or generosity. "Everyone walks around with a blue cornflower in their buttonhole here, to show respect for the king by way of his favorite flower," he wrote to his wife from Berlin on the king's birthday in 1879. He did not find the gesture endearing. "It is a leathery flower," he wrote, "being blue is its sole virtue. It has no fragrance, no beauty, no poetry. It is as though it had been designed specially for us; all that's missing on it is a red trouser stripe" (p. 291).

Yet an air of tolerance, Stechlin's permanent question mark, pervaded all these comments, no matter what side Fontane's shifting moods put him on. As a young man, Fontane had been a tutor (one of the several occupations covered by the "etc." of the suggested alternate title) in the Wangenheim household. There, he was particularly impressed by something Dubslav von Stechlin would have loved. Baron von Wangenheim, a senior official in the Prussian government, subscribed to the archconservative *Kreuzzeitung*, his wife to the rather more liberal *Germania*. "But what either of them really liked was to read the other's paper" (p. 146).

It was to be Fontane's attitude too, and it is good to read about it again in this fine biography.

JOACHIM REMAK
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MARTIN GREEN. *Mountain of Truth: The Counterculture Begins; Ascona, 1900–1920*. Hanover, N.H.: University Press of New England, for Tufts University Press, Medford, Mass. 1986. Pp. 287. $19.95.

This book narrates one aspect of the so-called German life-reform movement, which sought to regenerate men and women through vegetarianism, antialcoholism, natural healing, land reform, and communal settlements. Life-reform was a response to the rapid growth of cities at the *fin de siècle*, which seemed to endanger bodily and spiritual health by alienating people from nature. The name Monte Verità was given to a hill in the Swiss village of Ascona by vegetarians who built a natural-cure sanitarium there in 1900. For the next twenty years some one thousand people who wanted to opt out of society and point the way to a new paradise lived for various lengths of time in huts or houses on the mountain. Martin Green's book concentrates on three of them: Otto Gross, who tried to integrate psychoanalysis with anarchism and the counterculture, Rudolf von Laban, one of the creators of the modern dance, and Gusto Gräser, guru and prophet, who vitally influenced his close friend Hermann Hesse as well as the German Youth Movement. The first part of the book unravels these three lives; the second is devoted largely to brief sketches of what happened year by year on the mountain, centered on the book's principal characters. The third part of the book attempts to trace the "Asconian Idea" through contemporary art, culture, and politics.

The book focuses on individual personalities, how they lived, interacted, and what they wrote. A concise bond between such men and women is difficult to find; for example, Green's main characters, so we are told, never spoke to each other. Green provides some cohesion to his book through emphasizing the Asconians' joy in their newly found bodily freedom: its movements and erotic potential. Oddly enough, nudism, which was an important component of this freedom, is never mentioned, nor is the mountain's most remarkable structure, Eliasar von Kupfer's *Elisarion* filled with paintings of nude St. Sebastians and androgynous youths. The book fails to encompass the wide variety of Asconian ideals ranging from Gräser's simple-minded rejection of all political thought to Erich Mühsam's anarchism.

The dance best captured the "life-body-movement-expression" (p. 190), which Green considers the essence of the Asconian ideal and which, through the talents of von Laban and his pupil Mary Wigman, became Monte Verità's most successful export. However, the attribution of influence is not always convincing here, for it seems to depend entirely on personal contacts and parallelisms. Almost certainly, writers such as Hesse and even D. H. Lawrence came directly under the spell of Monte Verità, but that Franz Kafka or the painter Emil Nolde knew about the mountain seems doubtful. The specific influence of Ascona is never properly disentangled from the general influence of the life-reform movement. The analysis of the Asconian ideal in politics is the weakest section of the book because the author's single-minded emphasis on personalities neglects the social and historical setting. For example, we read about Gross's interaction with others on the mountain, but we do not learn about his effort to redirect the new science of psychoanalysis from support to opposition to society and its establishment. Moreover, the parallelism between Asconian political attitudes and Ghandi's thought, emphasized here, is much less important than the use made of their visions by *völkisch* thinkers. Green is aware of such cooptation, yet he assigns to the publisher Eugen Diederichs a place on the

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mountain and ignores the basic *völkisch* nature of his thought. Von Laban and Wigman made important contributions to the liturgy of rightist mass movements. They would have liked to contribute to the festivals of the Third Reich, but the regime itself prevented their participation.

Those who want a full account of who was who on Monte Verità will have to turn to the catalogue of the exhibition of 1978, *Monte Verità, Ascona* (1978). Meanwhile, Green's book provides a good and highly readable introduction to a remarkable experiment in which men and women, each in his or her own way, attempted to put into practice their individual visions of a life lived against the dictates of society and the passage of time. Monte Verità should be remembered not solely for its bizarre characters or literary influence but also for ideals that contributed to a counterculture eventually absorbed by society through health-reform and ecological movements. But then, on the mountain itself a luxury hotel has taken the place of the huts of the apostles, and the legacy of the men and women who lived there must now be sought in the very society they sought to escape.

GEORGE L. MOSSE
University of Wisconsin and Hebrew University

MERITH NIEHUSS. *Arbeiterschaft in Krieg und Inflation: Soziale Schichtung und Lage der Arbeiter in Augsburg und Linz 1910 bis 1925*. (Veröffentlichungen der Historischen Kommission zu Berlin, number 59; Beiträge zu Inflation und Wiederaufbau in Deutschland und Europa, 1914–1924, number 3.) New York: Walter de Gruyter. 1985. Pp. xiv, 308. $44.50.

The social and economic dislocations war and hyperinflation caused in Central Europe are receiving renewed attention as fundamental causes of political instability in the 1920s. In this study Merith Niehuss describes the social and demographic impact of rapidly changing economic circumstances between 1914 and 1925 on Augsburg and Linz and thus makes a localized comparison of broader problems in Germany and Austria. Part 1 uses samples drawn from address books to trace social stratification and mobility. The results here are unsurprising: the more dynamic economy of Augsburg permitted greater upward social mobility than did the economy of Linz, although the great majority were immobile through the entire period. Part 2 shows that inflation and the resultant decline in real income of workers caused a variety of indicators of social pathology to increase or peak in the early 1920s: unemployment, alcoholism, murder, robbery, mortality in hospitals, suicide. Unfortunately, the lack of connection between these two sections (mobility and the sample are never mentioned in part 2) and the overabundance of quantitative data prevent this work from rising much above the level of a data handbook.

The number of numbers here is overwhelming: 136 tables and 18 diagrams make for a headlong race through poorly explained data. Numerous tables are mentioned in only one sentence. Many tables are poorly constructed, omitting crucial comparative data or total population figures. Extended discussions of some methodological questions underscore the lack of attention to actual data. After a ten-page comparison of several indices of price inflation, the discussion of the inflation itself covers only two pages (excluding tables). Yet other key methodological questions, especially the use of backwards linkage, are barely addressed.

The brief explanations of tables are frequently misleading: the small difference between Augsburg and Linz in mobility among social classes (16 percent versus 19 percent) is exaggerated (p. 57); twice Niehuss claims that occupational mobility was higher in 1914–21 than in 1921–25 (pp. 77, 85), but the difference can be explained by the differing time spans; the data on page 186 directly contradict the claim that living conditions in Linz were responsible for higher tuberculosis mortality.

Some of the data are fundamentally weak. As the basis for samples tracing social mobility, address books are notoriously unrepresentative of the working class. At the least they need to be constantly rechecked against census data, yet Niehuss bases claims about female occupational distribution on her tiny sample of 166 Augsburg women (pp. 50–51). Her discussion of "structural mobility" (pp. 54–62), occupational change resulting from the evolving distribution of occupations, is based only on the samples of persons linked by address books, which she acknowledges is unrepresentative of total populations.

For specialists in Weimar social history, however, the book offers much of value. Niehuss assembles a wealth of data on the effects of economic dislocation. Her choice of period, spanning the end of the *Kaiserreich* and the early years of Weimar, indicates the prolonged socioeconomic agony that accompanied the introduction of democratic systems in Central Europe. When combined and compared with other local data, Niehuss's statistics could take on greater meaning than they achieve in this unsophisticated work.

STEVE HOCHSTADT
Bates College

WOLFGANG ZORN. *Bayerns Geschichte im 20. Jahrhundert: Von der Monarchie zum Bundesland*. Munich: C. H. Beck. 1986. Pp. 790. DM 78.

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For much of its long and rich history Bavaria was an independent entity and a European power in its own right. Throughout the twentieth century it has been a part of Germany and contributed significantly to that country's political, economic, and cultural evolution. The modern history of Bavaria is, in other words, very much part of the "German problem."

The Bavarians themselves have often seen their history in a somewhat different perspective. Rather like their neighbors to the east, they have tended to regard themselves as the victims rather than as the makers of recent German history. And, again like the Austrians, they have needed to look only to the "Piefkes" in Berlin to find the source of most of their troubles.

Fortunately, Wolfgang Zorn presents Bavaria as a key actor in—not a victim of—recent German history. He examines the contributions (for both good and ill) that Bavarians have made in virtually every aspect of German life, from high-level politics and economics to social trends, military affairs, culture, and technology. We even learn that a *Münchnerin* teamed up with a Saxon to win the Olympic gold medal in pairs figure skating in 1936: significantly enough, they skated to the melody of *Deutschland, Deutschland über Alles*.

Bavaria's role in the Third Reich occupies a central place in Zorn's study, as indeed it must. The author, however, chronicles not just the well-known Munich origins of the Nazi party but also Bavaria's rather less exalted fate during the heyday of the movement when it was reduced to a *Reichsprovinz*, Hitler having moved from Munich's "Brown House" to the Reich Chancellory in rival Berlin. But, lest this provide fuel for the victim theory of Bavarian history, Zorn makes clear that, if Bavaria produced its share of anti-Nazi resisters (most notably the "White Rose" group at the University of Munich), it also continued to supply its quota of party ideologues and SS and SA thugs. Munich, the only German city to boast a swastika in its coat-of-arms, remained proud of its title "Hauptstadt der Bewegung" just as Nuremberg gloried in its role of host city to the Nazi party rallies and Bayreuth in its possession of the Richard Wagner *Festspielhaus*, which Hitler turned into Nazism's court theater. Only when Allied bombs began reducing Bavarian cities and towns to rubble did their inhabitants recall their affinity with Austria and insist on depicting the war as the latest in a long line of calamities visited on them by Prussian barbarism. (Since the war they have never forgiven the Austrians for successfully marketing this interpretation of Central European history whereas they could not.)

Students of the postwar era will welcome Zorn's substantial treatment of Bavarian developments during the Allied occupation and first two decades of the Federal Republic. If there has been any revenge for 1866 in German history it occurred during this latter period, when the *Freistaat Bayern* led a resurgent south Germany to economic superiority over the north. In politics Franz Josef Strauss, the "uncrowned King of Bavaria," emerged as a key figure in federal German politics. Culturally, of course, Munich prospered at the expense of divided and isolated Berlin; although it never regained the cultural preeminence it enjoyed at the turn of the century, it became an important center for publishing, music, and (especially) film. Even in military affairs, long a bastion of the Prussians, Bavaria quickly assumed a leading role. A Bavarian, Richard Jaeger, chaired the Security Committee in the Bundestag; Josef Kammhuber, another Bavarian, became the new Luftwaffe's first inspector; and Strauss himself became defense minister in 1956, holding that office until the *Spiegel* affair of 1962 drove him (temporarily) out of Bonn.

Zorn's study not only examines Bavaria's relations with the rest of Germany and with Europe but also chronicles developments within that state. He provides a great deal of information about Bavarian Catholicism and Franconian Protestantism and about social and cultural life not just in Munich but also in smaller Bavarian cities such as Augsburg, Nuremberg, and Regensburg. We learn much about marriages and social connections within the Bavarian nobility. The state's rise to industrial prominence is especially well treated as are important developments in agriculture. The author does not ignore the somewhat separate history of the Bavarian Palatinate. Discussion of these matters is no doubt Zorn's greatest service, for single-volume histories of modern Bavaria rarely go much beyond the perspective that might be gained by climbing the towers of the *Frauenkirche*.

But breadth of scope and richness of detail are also a problem here because Zorn has failed to present his abundant material as effectively as he might have. The book would certainly have been strengthened by a more rigorous thematic organization or perhaps by the addition of an introductory chapter setting out the central themes. As it stands, the reader must have considerable orienteering skill to negotiate this twisting and boggy path from the *Prinzregentenzeit* to the end of the Adenauer era.

DAVID CLAY LARGE
Montana State University

DIETRICH ORLOW. *Weimar Prussia, 1918–1925: The Unlikely Rock of Democracy*. Pittsburgh: University of Pittsburgh Press. 1986. Pp. xii, 363. $32.95.

Dietrich Orlow, known for his detailed history of the Nazi party, deals here with the structure and func-

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tioning of the Prussian state government in the critical early years of the Weimar Republic. His book appears at the same time as the lengthier treatment of Horst Möller, *Parlamentarismus in Preussen* (reviewed in the *AHR* 91 [1986]: 687–88). The two books are very different, but they reach much the same conclusions: that Prussia's democratic government was far more successful than the Reich's and that it served as a worthy prototype of the Federal Republic of today. Orlow's study is based on extensive use of archival and parliamentary collections and effectively draws on a number of memoirs and diaries. Sometimes, however, it betrays a certain lack of broad background in German history: for example, in translating the name of Prussia's core province as the "Marquis of Brandenburg" (p. 91) and in referring to the Christian Social movement of "Paul" Stöcker (p. 121).

The Kapp Putsch of 1920 is the turning point that divides Orlow's discussion of political parties, the formation of cabinets, and problems of personnel and administration. He finds that the defeat of the putsch encouraged the democrats and, what is most important, persuaded the leadership of the Prussian branch of the German People's party to abandon its support of the German Nationalists and agree to participate for three years in a cabinet of the Grand Coalition. This in turn strengthened the state government during the crises of political assassinations and the French occupation of the Ruhr. The relative reasonableness of the People's party in Prussia was matched by the cooperation of the other key parties, the Social Democratic party, led in Prussia by its moderate right wing rather than by the left as in the Reich, and the Center party, led in Prussia by its left wing rather than the right as in the Reich. In the author's words, "Pragmatists rather than ideologues dominated the state's political scene" (p. 8).

Another factor in Prussia's success was that the cabinet ministers were professional party politicians from the Landtag rather than "unpolitical" figures such as Wilhelm Cuno and Hans Luther in the Reich government. Unaccountably, Orlow does not stress a related factor that is central to Möller's argument: that in Prussia the Landtag had the sole constitutional responsibility for choosing the minister-president and for resolving party conflicts, whereas, in the Reich, the decisive role of the president contributed to the fatal weakness of the Reichstag. Thus, that Prussia "enjoyed consistent political stability under the same political system" as the Reich is not strictly true (p. 8). Furthermore, Orlow's descriptions of endless party bickering and agonizing over the formation of coalitions demonstrate that Prussia was "stable" merely in a comparative sense. He points with pride to "only" six ministries and four prime ministers in the state in seven years and dismisses the two ministries of Adam Stegerwald and Wilhelm Marx as brief transition governments; he forgets that they were "transitional" only in hindsight, not in intention. The state was a better "rock of democracy" in the later period from 1925 to 1932.

Orlow is not completely convincing in his insistence that the Weimar leaders of Prussia were inspired by the state's special historical identity and shared purpose (which he labels, oddly, "étatisme"). He has shown, however, that the existence of a well-governed and relatively stable Prussia was crucial in enabling the Weimar Republic to survive its first six years, and he has provided a useful and worthwhile counterpoint to its history at the national level.

ELLEN L. EVANS
Georgia State University

ERIC G. REICHE. *The Development of the SA in Nürnberg, 1922–34*. New York: Cambridge University Press. 1986. Pp. xviii, 314.

Researchers who have dug down into the mire of local and regional Nazi party organizations owe a debt to Eric G. Reiche, whose doctoral thesis of 1972 on the Nuremberg SA was the first detailed local study of the storm troopers. Now, after an unfortunately long interval, Reiche's study has been published. What does it offer?

Reiche presents a blow-by-blow account of the SA in Nuremberg from its beginnings until the purge of 1934 and provides detailed tables outlining its growth and social composition on the basis of records of 358 former storm troopers. It contains few surprises: the Nuremberg SA was involved in political violence similar to that throughout Germany as the Weimar Republic collapsed; it played a key role in recruiting activists to the Nazi movement; it contained a fair number of workers but "appealed to virtually all sectors of the city's male population" (p. 229)—including professional people; and it was involved in a series of conflicts with the Nazi party organization (culminating in the "Stegmann revolt" shortly before Hitler came to power).

Reiche has produced a well-crafted monograph, but the story he tells already is quite familiar. Yet, in many ways, Nuremberg offers a particularly fascinating place in which to study violent Nazi politics. It was an important industrial center; it was a major stronghold of the Social Democrats; its politics (as Reiche notes in the introduction) were colored by antipathy toward Munich (the seat of both the Bavarian government and the Nazi party headquarters); and it was Julius Streicher's stomping ground. Although the Nazis and Communists frequently fought in the streets, the most inviting targets for Nazi attacks were the Social Democrats—with their

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impressive organizational network of newspapers, trade unions, and party cells. The Social Democratic party was identified with the republic, with authority, with the "system"—in few places more so than in Nuremberg. Did this give a peculiar character to SA activities and recruitment in the city or to the relations of SA men with their neighbors and colleagues? How did the Nazis organize on the streets and in neighborhoods within a predominantly Social Democratic political environment? Unfortunately, such questions remain unasked and unanswered.

One problem with the book is that Reiche tends to take his sources a bit too much at face value. He informs us in detail of the expressed sentiments of SA leaders and men, for example, their "feeling of superiority over the party" (p. 134) and their anger at Streicher's behavior and corruption. But why did SA leaders use the language they did, and to what extent did they speak for the membership? We learn a lot, for example, about the leadership's intentions for the special SA formations, but what did these formations actually do? Why did the young men who became storm troopers want to prove their toughness in this fashion and play soldier? And what does all the tabulated data actually tell us about why people involved themselves in this peculiar violent brand of politics? On this last point, Reiche is wisely cautious, but at times his caution is overwhelming. The result is a book that is strong on description, but readers looking for new insights into the rise of the Nazi movement or the nature of political violence in Weimar Germany will be disappointed.

RICHARD BESSEL
Open University

ALICE GALLIN. *Midwives to Nazism: University Professors in Weimar Germany, 1925–1933*. Macon, Ga.: Mercer University Press. 1986. Pp. viii, 134. $16.95.

If you missed the debate fifteen years ago about the role of German university professors as precursors of Nazism, you can now read a potted version of several of the books (if not of the debate itself). A conversation with Fritz Ringer in 1971 spurred Alice Gallin to take up her pen, and the journalist Hans Peter Bleuel suggested the term "midwives" in her title. This study has more the character of a brief essay than of a work of original research. The only justification for such a publication would be if it brought a fresh perspective to the question. It does not. In fact much of the recent literature is not even mentioned—80 percent of the articles cited in the bibliography are over twenty years old.

Why did professors not halt the rise of Hitler? Gallin seeks the answer in professorial thought and university structure in the Weimar period. She tries to draw up a profile of German academic values by examining the narrow base of public speeches of full professors at the universities of Bavaria and Württemberg. A lack of precision in their usage of terms such as "Reich," "Führer," and "Kulturmission" draws her criticism. Gallin finds the same language in the later speeches of Hitler and Goebbels. Post hoc, ergo propter hoc?

The author's insistence that this common rhetoric is the key to understanding the smooth *Gleichschaltung* of universities in 1933 represents an overly intellectualized interpretation that is too far removed from the real world. She accuses the professors of seeing no need to protest the removal of their Jewish colleagues. Her main documentary sources for gauging the atmosphere of the university, however, are the contemporary reports of U.S. State Department officials to Washington, which must be second- or third-hand accounts at best. Ideological concurrence was not the whole story. Physical violence threatened German professors, and they acquiesced in the Civil Service Law partly on the understanding that indiscriminate action and firings would be brought under control by the government.

In a number of other instances Gallin appears insufficiently conversant with the literature to interpret matters correctly. She asserts that Kurt Huber, who was executed with the White Rose Resistance group, joined the Nazi party in 1940 "out of fear of communism." Actually his wife signed him up without his knowledge in a (successful) attempt to increase his meager salary, as Christian Petry revealed in 1968. Gallin's interview in 1972 with the "distinguished-looking . . . gracious, thoughtful" Albert Speer in his "charming family home" evidently dulled her critical faculties. Speer told her that Hitler often remarked that he had no problem obtaining the cooperation of the universities. Yet Hitler both publicly and privately expressed his frustration with academics. If Hitler really did make the remark (which is open to doubt), then its context would have to be sought. Gallin should not have relied on this comment to prove Hitler's overall satisfaction with the professoriate or the identification of academics with the National Socialist movement. That has long been known not to have been the case.

GEOFFREY J. GILES
University of Florida

ANNA BRAMWELL. *Blood and Soil: Richard Walther Darré and Hitler's "Green Party."* Bourne End, Buckinghamshire: Kensal. 1985. Pp. viii, 288. £12.95.

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Born to German parents in Argentina in 1895, Richard Walther Darré became the leading Nazi spokesman for a "blood and soil" agrarian ideology. After successfully coordinating Nazi infiltration of traditional German farmers' organizations between 1930 and 1933, Darré assumed a number of important positions in the Third Reich, ranging from minister of food and agriculture to national peasant leader and head of the SS Race and Settlement Office. Although Darré's role in Nazi agrarian politics has attracted considerable attention from such scholars as Horst Gies, Gustavo Corni, and J. E. Farquharson, Anna Bramwell has written the first comprehensive, scholarly biography of Darré. Bramwell effectively used the papers of Darré and Herbert Backe and extracts from the diary of Darré, which was destroyed in the late 1960s. The only major weakness in Bramwell's research was her failure to consult the SS files in the Berlin Document Center.

This is a thorough, if at times too sympathetic, survey of Darré's ideology and political activities. Darré, a trained agronomist with practical experience in the Baltic region, wanted to protect the Nordic peasantry, the life source of the Germanic nation, by ensuring the economic viability of peasant farming and encouraging a "eugenically-oriented racialism" (p. 201). Bramwell distinguishes between Darré, the "racial tribalist," and Heinrich Himmler, the "imperialist with romantic racial overtones" (p. 130), although she admits that Himmler implemented many of Darré's ideas in a perverted way. Also, Darré supported the resettlement of "German lands" in the Baltic region and in Poland, but he opposed Hitler's invasion of Russia. According to the author, Darré's power began to wane after 1936 because he remained true to his original ideas. In 1938 he was removed as head of the SS Race and Settlement Office, and in 1942 he was dismissed as minister of agriculture after having lost most of his real authority to Backe.

Unfortunately, the book is marred by a number of minor problems. Carl Goerdeler is described as the former mayor of Danzig instead of Leipzig; Günther Pancke, Darré's successor as head of the SS Race and Settlement Office, is referred to as Gustav Pancke; and the SS Race and Settlement Office is discounted as a "cardboard edifice" without adequate explanation of its organization. Contemporary reports of the Sicherheitsdienst also make clear that Bramwell is incorrect in asserting that the official explanation of Darré's dismissal in 1942, as merely a temporary leave because of illness, was widely accepted by the public. More serious are the author's contentions that between 1919 and 1931 "agrarian ideology seems not to have touched Himmler's political life" (p. 133) and that "Hitler's need for the rural vote was based on a shortfall in the 1930 vote" (p. 83). In fact, Himmler helped organize Nazi rural propaganda before 1928, and the "shortfall" occurred in 1928, not 1930. Finally, are we really to believe that without Darré "the ecological movement would have perished in his time and place" (p. 200)?

JOHNPETER HORST GRILL
Mississippi State University

FRANCIS R. NICOSIA. *The Third Reich and the Palestine Question*. Austin: University of Texas Press. 1985. Pp. xiv, 319. $35.00.

Few countries during the early twentieth century concerned German policy makers less than Palestine. First an Ottoman outpost and after 1919 a British mandate, Palestine was a mosquito- and disease-ridden swamp with approximately two thousand German Christians living amiably among their Jewish and Arab neighbors. During the early years their influence extended to a few scattered German schools, hospitals, and orphanages and ten Protestant Temple Society settlements. German imports from Palestine were infinitesimal. Berlin had no territorial interests in the area and, in fact, had long conceded the Middle East to British control. What little policy existed during the Weimar years was generally in support of the status quo and mildly sympathetic toward the embryonic Zionist movement. Like most Europeans, the Germans viewed the Arabs with contempt.

With the appearance of the Third Reich, policy toward Palestine—still marginal in German diplomacy—was governed by three factors. The first was the desire for British friendship and hence the continued policy of acquiescence to the status quo in Palestine. Until the Munich Crisis of 1938 shattered any future cooperation between Germany and England, Hitler carefully refrained from any official activity that could be interpreted as hostile to Great Britain. The second factor in Hitler's contradictory policy was the view that the Jewish problem could be solved through emigration, with Palestine as the most convenient destination. It was after failing to encourage such emigration that the Nazis embraced mass murder. The last factor was Hitler's general disdain for the Arabs. In all three areas, the Nazis found it most appropriate to turn for help, astonishingly, to the tiny Zionist movement in Germany.

Their cooperation began as early as August 1933 when Berlin signed a trade agreement, the *Ha'avara* or Transfer Agreement, with the Anglo-Palestine Bank. In a complicated arrangement Jews willing to emigrate to Palestine were permitted to transfer twenty thousand dollars in specific trade goods, which were to be resold to facilitate settlement in their new homeland. Thus began a curious rela-

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tionship in which the Nazis provided preferential treatment for German Zionists over other German Jewish groups, encouraged programs to retrain German emigrants bound for Palestine, and even participated in illegal efforts to smuggle refugees into Palestine after Britain closed the gates.

Germany's interest in Palestine, admittedly never of great importance, had its effect on various other issues as well. For example, since the five hundred thousand Jews living in Germany in 1933 (constituting a scant 0.76 percent of the total population) were overwhelmingly non- or anti-Zionist, the government's decision to work with the Zionists forced many to redefine their Judaism. Without question, the Nazis significantly bolstered the tiny Zionist movement, and one can marvel at the irony that saw the Nazis support the mass emigration of Jews to Palestine without drawing the logical conclusion that such increased immigration would inevitably strengthen the nucleus from which a Jewish state would emerge in due course. Germany's relations with Britain on the issue of Palestine were no less complicated than its dealings with the Arabs.

Francis R. Nicosia has written the definitive study of this fascinating but thoroughly minor epoch in the histories of the participants. It is a masterful examination of every interwoven thread in the complicated tapestry of Nazi Germany's relations with the Middle East, as well as with Great Britain and the Zionist movement—a mastery that the author immodestly trumpets at length in his preface. Still, Nicosia has written an impressive if overblown study; indeed, a full third of the manuscript, one hundred pages, is devoted to appendixes, elaborate footnotes citing records from a dozen archives on three continents, and a twenty-page bibliography (the only notable omissions being the works of Solomon Colodner and the prominent East German journalist, Klaus Polkehn). If the sheer weight of the information and the painstaking research does not convince the reader of the book's importance, perhaps the preface will.

ARNOLD KRAMMER
Texas A&M University

MANFRED KÖHLER. *Die Volksschule Harsum im Dritten Reich: Widerstand und Anpassung einer katholischen Dorfschule.* (Beiträge zur historischen Bildungsforschung, number 2.) Hildesheim: August Lax. 1985. Pp. 214. DM 40.

This is a history of the public elementary school of Harsum, a village about six kilometers north of Hildesheim and now in the *Land* of Lower Saxony in West Germany. Originally part of the territorial possessions of the bishop of Hildesheim, after the secularization of 1802–03 it became part of Hannover and in 1866 was incorporated into Prussia. The village was and is overwhelmingly Catholic; its school was organized on a Catholic confessional basis under the bishop, and its nature and orientation have not changed. Manfred Köhler has written a book designed for local readers; he provides details of school construction, biographical notes on teachers, and an unpaged annex of pictures of various school classes, the faculty, facsimiles of assignments, and other school matters.

Although the book is local history and brings no startling new material, it presents interesting details on conditions before and after the advent of Hitler. In a totalitarian state anything in opposition is in a sense resistance (*Widerstand*), and, outwardly, not much of this existed in Harsum. The word *Anpassung* (adaptation, accommodation) in the title really describes the school's response to the Hitler era. Here, it is excellent. The teachers were all practicing Catholics, but the male members of the faculty joined the party, only one of them with any enthusiasm. Emil Dorpmund, who headed the school from 1930 to 1945 and did not join the party until 1937, was most adept at steering a safe course for the school. Radios were set up so the students could listen to Hitler's speeches and the Hitler salute was used. The school complied with ceremonial raising of flags, observed decreed patriotic holidays, used new reading and mathematics textbooks introduced in 1935–37, enlisted students to collect old clothes, metals, and medicinal herbs during the war, and observed other party programs. The Catholic spirit and orientation of the school and its ties to the church were not changed. Religious instruction continued as before, and the conversion from a confessional to an interdenominational school (*Gemeinschaftsschule*) was stalled until May 1, 1942, although that program was launched in 1935. A few Protestant children always attended the school, the numbers varying from nine to fifteen during the 1930s. Little is said about these pupils although Protestant religious instruction for them began in 1904. Restrictions were placed on Catholic organizations after the conclusion of the concordat in 1933, but no great controversies arose in Harsum. Apparently, no attempts were made to remove crucifixes and religious pictures from the classrooms. Köhler also examines denazification procedures, particularly the case of Dorpmund. Parents voted 97.1 percent for the reestablishment of a confessional school after the war. Clearly, the loyalty of the inhabitants to their church and their ancestral piety prevented great inroads on the conduct of the school. The author stresses that instruction remained much as it had always been—*Alles blieb beim alten.* The book is well documented; there is a detailed table of con-

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tents, a bibliography, a useful two-page table of abbreviations, but, alas, no index.

ERNST C. HELMREICH
Bowdoin College

RUTH BETTINA BIRN. *Die Höheren SS- und Polizeiführer: Himmlers Vertreter im Reich und in den besetzten Gebieten*. Düsseldorf: Droste. 1986. Pp. x, 430. DM 48.

Ruth Bettina Birn has produced a masterly study of the office of higher SS and police leader and those who held this position in the period 1937–45. At the maximum there were thirty-seven positions of widely divergent power and function. Twenty-six men held just one such position; two men held four different higher SS- and police positions serially; three held three different posts between 1938 and 1944. Ten moved from one position to another. Moreover, some of the names are familiar in other SS roles: Karl Wolff, Ernst Kaltenbrunner, Richard Hildebrand, Otto Hofmann, Erich von dem Bach-Zelewski. So not only were the positions vastly different—internal Reich, Austria, Bohemia-Moravia, annexed eastern territories, General Government, Army Group A, Adriatic Littoral, Denmark, Serbia, military commander of Belgium–Northern France, and *z.b.V.* (on special assignment)—but the importance and employment of the men themselves were markedly different. Birn inclines toward systematization and generalization and strongly favors an "intentional" interpretation, with respect both to Himmler and Hitler and to the SS. Playing down improvisation and accident, the author stresses the way the conflict motif in National Socialism, and especially in the SS, was essential to its world view and was a method of human selection. Provided conflict led to greater destructive efficiency in the war against designated enemies (Jews, Slavs, "criminals," shirkers, sexual and mental deviants, partisans), the conflict always was backed by Himmler, and the victors were the most ruthless. Birn is quite conscious of the centrifugal tendencies in the SS and in National Socialism: the higher SS and police leaders owed their rise and power to the ever-unfolding chaos and internecine struggle, especially from 1941 to 1944.

This book has many strengths. It is clearly organized and written in a straightforward style. After an introductory survey of origins, legal and extralegal competencies, territorial structure, and finance, Birn provides an excellent functional analysis of the relationship of these factors not only to the SS and police but also to the major authorities of the Reich. If anyone can find rationality and logic in the creation of Heinrich Himmler, it is Birn. This is not intended ironically: she successfully winnows out the purposive from the improvised. Next, the author groups the various positions into a typology: colonizers of the east, *grossdeutsch* politicians, military administrators, foreign policy strategists, stiffeners in crisis, and home front guards. This section is much less analytical but, like the earlier parts, beautifully researched and illustrated. Finally, biographical and sociological profiles supply some important insights. These men were not "characteristic" Nazis: they ranged from blue blood to blue collar; they were not especially from the lower middle class, nor were they, in spite of their high SS rank (generals), very well educated. She finds their common denominator in their *Weltanschauung*, which they took very seriously. Technocrats? Yes, but they were also true believers. The higher SS and police leaders were not the exclusive executors of the SS: the Main Office chiefs formed a separate fraternity (although a few of the leaders had held this position); the concentration camp empire, the Waffen SS, and the Replacement Army after July 20, 1944, remained outside their grasp.

Birn has written an important study. All students of the SS will have to consult it, and students of National Socialism generally will gain from her perceptive handling of Nazi faith and style. The typescript format is a bit hard to read, but it is apparently without errors.

ROBERT KOEHL
University of Wisconsin

GERHARD HIRSCHFELD, editor. *The Policies of Genocide: Jews and Soviet Prisoners of War in Nazi Germany*. Foreword by WOLFGANG J. MOMMSEN. Boston: Allen and Unwin, for the German Historical Institute, London. 1986. Pp. xiii, 172. Cloth $22.95, paper $9.95.

This important, controversial work developed from seminars held at the German Historical Institute in London. Except for Falk Pingel's sensitive essay on resistance in the camps, it focuses on how the Holocaust originated and the participation by various groups within Germany. These essays, edited by Gerhard Hirschfeld, represent the scholarship of a generation of German historians who did not personally experience National Socialism and World War II.

Current scholarship on the Holocaust divides between "intentionalists" and "structuralists." Intentionalists see Hitler's ideological intent as decisive; structuralists minimize his role and emphasize the piecemeal emergence of the "Final Solution."

The essays by Christian Streit and Jürgen Förster reject attempts to draw a sharp dividing line between the activities of the SS and those of the army. The *Wehrmacht* accepted Hitler's definition of the

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war in Russia as an ideological one and shared his goal of the permanent elimination of Russia and bolshevism. It willingly carried out the infamous commissar order. Its participation in the killing of Russian prisoners of war (which Streit has documented in a previous book) consisted of handing over some five to six hundred thousand selected Russian POWs to the SS (commissars and functionaries, agitators, fanatical Communists, and all Jews) as well as the systematic starvation of prisoners under their control (out of 5.7 million, 3.3 million perished). This has received scant attention in historiography of the Holocaust or World War II.

Lothar Kettenacker addresses the issue of whether the mass killings were due to "a single, incomprehensible fanatical will [of Hitler] . . . or did the Jews get caught up in the machinery of a system of power . . . that had gone out of control" (p. 73)? He notes that individuals sought a rational justification for their murderous acts. Himmler declared that the Jewish question was "a considerable factor in the internal security of the Reich and Europe," and Hans Frank observed that if the Jews had not been eliminated it would have been "like sitting on a volcano, which could have erupted at any minute" (p. 88). Kettenacker also refers to the "confusion among the traditional conservative elites about the priorities of justice and legality" (p. 88).

Hans Mommsen, at the center of the current historiographical controversy, rejects the existence of an order from Hitler for extermination (for which Kettenacker cites some evidence). "The idea that Hitler set the genocide policy in motion by means of a direct instruction can be completely rejected" (p. 127). Mommsen uses the absence of such an order to explain the mass repression of knowledge of genocide by individuals (such as Albert Speer) in influential positions. If not Hitler, then who? "The utopian dream of exterminating the Jews could become reality only in the half-light of unclear orders and ideological fanaticism. . . . The process developed its own internal dynamic" (pp. 127–28). This, Mommsen argues, makes it impossible to assign responsibility to a few individuals.

GEORGE M. KREN
Kansas State University

EARL R. BECK. *Under the Bombs: The German Home Front, 1942–1945*. Lexington: University Press of Kentucky. 1986. Pp. xi, 252.

The history of war provides fascinating insights into the popular concept of armed conflict. Accounts of national violence often present tales of individual heroism, collective success, and official rationalization. Such presentations neglect the human realities of modern struggles: the aimless slaughter of innocents, the constant battle for survival, the omnipresent controls of governments, and so on. Hiding or avoiding such realities contributes to the continuance and acceptance of violence as a viable aspect of the human condition.

Earl R. Beck's useful volume provides an insightful description of Germans within the construct of a dreadful war. His study seeks a balance between the large-scale deteriorating conduct of war, the governmental efforts for control of the home front, and the individual struggles for survival. He takes no moral position concerning the issues because judgment is not part of his purpose. He wants understanding and awareness of individuals caught up in the holocaust of war—irrespective of their personal guilt or innocence. His narrative provides a dispassionate account of the circumstances in Germany and the hapless decline into the chaos of 1945. It was a terrifying descent filled with fear and, ultimately, horror.

Relying on a careful scrutiny of published materials and some documentary sources, Beck provides a well-written, well-organized account, which reflects his professional skill and long experience. He adds an excellent portfolio of pictures as a complement to the text. In this sense his effort merits approbation for intent, readability, and conciseness.

As long as he keeps to the theme stated in his title, Beck does well. When he describes the more complex national efforts, both in the realm of military operations and political controls, he is less successful. The length of his book precludes careful attention to these areas, and his basic purposes make the meld between them and the individual realities of the home front awkward and uneven.

With that caveat aside, one must compliment Beck for his sensitivity. His book provides excellent reading and learning. One can recommend it as a powerful study.

CHARLES BURDICK
San Jose State University

RONA GOFFEN. *Piety and Patronage in Renaissance Venice: Bellini, Titian, and the Franciscans*. New Haven: Yale University Press. 1986. Pp. xvii, 285. $40.00.

Rona Goffen attempts to show how Franciscan piety and lay patronage affected the creation of three masterpieces of Venetian Renaissance painting. She brings to the method of contextual art history an admirable range of documentary, theological, and visual material. The result is a provocative, if uneven, assessment of art as an expression of cultural values in Renaissance society.

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Goffen includes five chapters with two appendixes and illustrates the text with four color plates and fifty-nine black-and-white photographs of varied quality. She begins with the history of the Conventual Franciscan church of Santa Maria Gloriosa dei Frari, where all three altar paintings remain in their original locations. Goffen proposes that this fourteenth-century priory serves as a microcosm for Venice's art, the Franciscan order, and the relationship of both to the state.

The next three chapters examine each painting: Giovanni Bellini's Frari triptych (1488), Titian's *Assumption of the Virgin* (ca. 1516), and his *Pesaro Madonna* (ca. 1519). She argues that the altarpieces interrelate on varied levels. What is most important, she believes that each celebrates the Franciscan devotion to the cult of the Immaculate Conception of the Virgin encouraged by the Franciscan pope Sixtus IV (1471–84). She also argues that these paintings reveal how the patrons linked their careers to the civic values of Marian devotion in Venice. The final chapter, in fact, problematically suggests that the Venetians celebrated the specific Marian cult of the Immaculata.

The book is most convincing when Goffen discusses the Pesaro family's art patronage. Finally, we can appreciate Bellini's altarpiece as the center of the combined sacristy-funerary chapel decorations, which, unexpectedly, commemorate a woman, Franceschina Tron Pesaro. Commissioned by her four sons, the painting shows their name saints flanking the Virgin and Child and includes inscriptions that apparently celebrate the feast of the Immaculate Conception. Goffen persuasively identifies the family portraits for Titian's *Pesaro Madonna*. In perhaps the book's best section she demonstrates that the rivalry between the family's admirals, Jacopo and his cousin, Benedetto, affected the art they ordered to memorialize their naval triumphs for the Serenissima.

The book is least persuasive when it argues for the specific meaning of the Immaculate Conception on the basis of paintings that offer equally plausible support for the prevalent celebration of Mary as *mediatrix*. Although the animosity between Franciscans and Dominicans about Mary's immaculate conception poisoned the atmosphere of the mendicant community, insufficient visual evidence remains for this period to claim that their respective positions determined artists' representations of the Assumption. Titian's *Assumption* distinguishes itself most by its spectacular color and internal light not by its approximation to a fifteenth-century sermon by Lorenzo Giustiniani. Moreover, Filippino Lippi's *Assumption* in the Dominican church of Santa Maria sopra Minerva in Rome does not, like Titian's painting, allude to both the Coronation and the Assumption. Filippino's angel carries a tamborine, not a crown. The Dominican theologian Vincenzo Bandello de Castronovo, by the way, did not spell his name Bandinello.

Although this book does little to suggest the artists' individuality and creative power, it helpfully places their work into a richly documented context of Franciscan piety and both secular and religious patronage in Renaissance Venice.

GAIL L. GEIGER
University of Wisconsin

AURELIO CERNIGLIARO. *Sovranità e feudo nel regno di Napoli, 1505–1557*. In two volumes. (Storia e Diritto, number 11.) Naples: Jovene. 1983. Pp. xxxix, 386; 390–1070.

Through his study of the kingdom of Naples in the first half of the sixteenth century, Aurelio Cernigliaro examines one of the main problems in the history of the modern state: the final outcome of centuries of strife for political and social power between feudatories and the crown. In Naples an unusual solution was adopted. After the kingdom was annexed to the Spanish crowns, the king of Naples had access to vastly superior military forces and possessed diplomatic and political advantages that gave him independence from feudal agreement and internal support.

The originality of Cernigliaro's contribution lies not so much in his study of the problem itself as in his minute analysis of the juridical and political methods the monarchy employed to gain primacy and subdue feudalism. But this analysis does not aim to build any particular theoretical or systematic pattern of the growth of the modern state. Juridical history, as seen by Cernigliaro and the Neapolitan school headed by Raffaele Ajello, is a history of social struggles, political conflicts, opposition of ideas, and contrasts between private or institutional elites; the law only reflects the tensions and the balance between political and social process.

In the kingdom of Naples the outcome was the preeminence of the *ministerium* or *togati*, that is, the clerks of the new state resulting from the victory of the crown. This development had nothing to do with the classic alliance between the monarchy and the bourgeoisie, and in this instance the mechanism of bureaucracy was not a sociological definition or delimitation. The ministerial structure of government was the final stage of the ancient *curia regis*, now transformed into a social milieu of jurists, *gens de lettres*, and noblemen, whose familiarity with the technical formulas that were frequently employed in the new political and social situation made them indispensable to the king. Cernigliaro indicates that the reform of the *Consiglio Collaterale*, the council set up by Ferdinand the Catholic in 1507 as the main

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support of the viceroy, was the crucial element in the formation of a new political leadership. Under Charles V the *milites* of the *Collaterale* were excluded from the council except when military matters were under discussion.

The course of events was extremely complex and cannot be summed up as a simple antithesis between the king and his vassals. In 1508 and 1547 the Spanish kings unsuccessfully tried to introduce the Spanish Inquisition into the kingdom of Naples, even though in 1547 the viceroy, Pedro de Toledo, seized the occasion to weaken the nobles' position in the city's main governing body. Cernigliaro's thesis regarding the events of 1547 is original, but he also provides evidence of the close bonds between barons and *togati* and, above all, of the persistence of strong feudal power in the provinces. One of the best aspects of Cernigliaro's study is the description of the way in which the *togati*, owing to their position as *extra* or *super partes* as the king's *longa manus*, assumed an intermediary role between the barons and the new balance of power.

In Cernigliaro's view the parliament was not only a means of feudal resistance and reinforcement but also a crucial and delicate point in the whole system. According to the new evidence examined by Cernigliaro, the parliamentary sessions in 1556 and 1557 reveal that these years were crucial in the establishment of new roles and status. The real power behind royal despotism was the *togati*. The dismissal of Toledo after the failure of his attempt, in 1547, to establish the king's personal rule by introducing the Spanish Inquisition reveals the real course of events: the baronage was strong only when it was allied with the *togati*, which was the case in 1547.

The estates had not yet emerged, but this development definitely marks their beginning. In Cernigliaro's book the importance of this social pattern and its relationship to the ministers in power do not emerge very clearly, but the configuration of the feud in the new regime is persuasively illustrated by an original analysis of feudal investitures. The formulas employed in drawing up these deeds—almost always completely neglected by historians—are impressive and enlightening. The concept of power and its social articulation are clearly seen in relation to the new situation of the kingdom.

The same can be said for another neglected deed contained in Neapolitan public law: the *capitulationes* between the king and parliament and other institutions, which reveal—particularly under Philip II—a reciprocally advantageous agreement.

On one hand, royal absolutism was constantly confirmed and the viceroy's power was strengthened to the advantage of the *togati*. On the other hand, the *togati* and the other orders that made up the kingdom supported a tendency toward autonomy from Spain and in so doing achieved an internal equilibrium, even during their frequent conflicts. In this way dependency on a foreign power was counterbalanced (although obviously not reversed) by the vivacious and complex traits of a country rich in cultural and human vigor.

As the reader may easily conclude, the beginning of the *respublica togatorum* in sixteenth-century Naples reveals a tendency to assume the aspect of a conservative regime.

In Cernigliaro's view, the ennoblement of the *togati*, which became an accepted custom, is the best evidence of their success, sanctioned by their penetration into institutions that were once the strongholds of the artistocracy. In this case a less optimistic judgment may be necessary to discern the sense not only of Neapolitan modern history but of that of the whole of Europe, which Cernigliaro's book invites us to do. The great importance of this book lies both in its several new ideas, based on the author's rather unusual exploration of documents, and in his evident interest in the development of the modern state in Europe.

GIUSEPPE GALASSO
Rome, Italy

FRANCESCO BENIGNO. *Una casa, una terra: Ricerche su Paceco, paese nuovo nella Sicilia del Sei e Settecento.* Catania: Cooperativa Universitaria Editrice Catanese di Magistero. 1985. Pp. 215.

One of the more original responses to the increasing difficulties experienced by the seigneurial economy toward the close of the long "Mediterranean century" developed in Sicily. Known as internal "colonization," this movement saw the foundation by aristocrats of new "feudal villages" devoted largely to the cultivation of cereals, made increasingly profitable thanks to recent population growth both in Sicily and overseas. One such settlement was Paceco, founded in the early seventeenth century by Don Placido Fardella, an enterprising and well-connected member of a rapidly rising noble family from the nearby city of Trapani. Following the initial construction of housing and public buildings by Fardella's agents, village land was divided into two categories: a latifundium subjected to direct seigneurial control, which included most of the arable land and almost all pasturage, and a lesser group of minifundia, or small peasant holdings leased through emphyteusis or quitrents. Success in establishing a colony depended on achieving a delicate balance between offering incentives sufficiently enticing to lure new inhabitants and levying enough dues to assure recovery of the initial investment as well as ongoing seigneurial profits. Unfortunately

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for Fardella and his successors, the colony of Paceco apparently failed to live up to expectations.

The bulk of Francesco Benigno's study consists of a brisk but thorough analysis of local population trends and property structures. Comparison of the *riveli*, the periodic censuses long a favored source of students of Sicilian demography and economy, reveals growing stratification within Paceco during the seventeenth and eighteenth centuries. A widening gap separated a small nucleus of *burgisi*, or middling property owners, from a much larger stratum of inhabitants possessing little or no fixed wealth who were obliged to seek day labor on either the baronial demesne or the holdings of their more prosperous neighbors. Benigno also argues that the fundamental unit of production and reproduction in the village was the nuclear family, characterized by a relatively low age at first marriage and neolocal residence. In short, although landholding patterns in Paceco conformed from an early date to a generalized trend toward the markedly unequal distribution of property, local family and household structures differed dramatically from the "Mediterranean family" model beloved by earlier generations of historians and ethnographers.

Internal colonization has long interested historians of early modern Sicily. The pioneering phase of study, which centered largely around questions of feudal law and the impact of these aristocratic initiatives on the relations between the parliamentary estates, has now given way to a growing number of local studies of individual settlements and their founders. Despite several organizational weaknesses—especially the absence of a concluding chapter—Benigno's study of Paceco exemplifies the exceptionally high quality of much of the local history of early modern Italy. The strength of this historical tradition derives not only from intimate familiarity with small-scale topography and geography but also from a willingness to pose larger questions and to root "local knowledge" firmly within the broader context of early modern economic and social history.

JAMES S. AMELANG
University of Florida

GIAN PAOLO BRIZZI, editor. *Il catechismo e la grammatica*. Volume 1, *Istruzione e controllo sociale nell'area emiliana e romagnola nel '700*. (Cultura e vita civile nel Settecento.) Bologna: Il Molino. 1985. Pp. 436. L. 35,000.

The title for these six essays, edited by Gian Paolo Brizzi, is poorly chosen. They have little to do with catechisms per se, and more with the religious ambiance of the area under consideration. Loosely binding the contributions are two themes: the all-pervailing influence of Jesuit schools, even after the suppression of the society, and the general late eighteenth-century European tendency to combine moral and doctrinal instruction in the Christian faith with state insistence that pupils not only have sound doctrine but also *civiltà*, defined as preparation for living in a peaceful, productive relationship with other princely subjects.

P. Lucchi examines various educational theorists and their ideas about the purpose of education, which was not to turn out masses of people capable of reading, writing, and calculation but to effect a compromise between the Latin and vernacular schools. From D. Marchesini's essay one is inclined to conclude, despite his torturous but impressive accumulation of percentiles on calligraphy and literacy, that the results are fairly obvious even without statistical data. For example, literacy was more widespread in urban areas than in the countryside, and males had a higher literacy rate than females. The last four essays reiterate common themes: the stereotypical virtues of the *buon maestro*, religious or lay, and his unquestioned (mostly) authority over his pupils (P. Severini); the methods, both old and new, for teaching Latin and Italian grammar (R. Ballerini); the, again stereotypical, ways of teaching both manners and morals (C. Pancera). Since there were formulas for teaching and encouraging emulation, some fixed as early as the sixteenth century (principally by the *Ratio Studiorum*) and heavy borrowing by other religious orders and in turn by civic or lay schools, that educational objectives bore strong family resemblances is no surprise. The teacher was expected to be a model of devotion, modesty, self-discipline, and all the Christian virtues. If he were not and this could be proved, his dismissal was perfunctory. Surprisingly, these essays detect little French or Jansenist influence (other than the texts of Port-Royal). The Italians were evidently working from a native tradition.

The last contribution (M. Turrini and A. Valenti), despite its dual authorship, is perhaps the best in the collection. It shows plainly that habit, elaborately contrived ritual (chiefly centering about Marian sodalities), regular confession, and eucharistic reception (generally once a month) were as significant in habituating pupils to act as Christians as were formal catechisms. In other words Italy was in transition between an oral and a written culture with oral tradition still prevalent.

In the introduction Brizzi promises a second volume to deal with the influence of enlightened monarchs on education in Emelia and Romagnola. Considering the complicated political structure of eighteenth-century Italy and the difficulties this presents to historians, this collection is a brave attempt to put that history on a statistical basis.

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CARLO ZAGHI. *Potere chiesa e società: Studi e ricerche sull'Italia giacobina e napoleonica.* Naples: Istituto Universitario Orientale. 1984. Pp. xxviii, 754.

In October 1984 Carlo Zaghi signed the preface to this volume on Italy in the Jacobin and Napoleonic periods. These seven hundred pages represented the best of a scholarly output that spanned nearly sixty years and encompassed not only the history of pre-Risorgimento Italy but other topics as well, among them the history of Italian colonization in Africa.

Zaghi's lifelong love affair with archival research began when he was not quite twenty. A stringer for and later editor of *Gazzetta ferrarese*, he became intrigued with the history of the Este dynasty and its neighbors and rivals, the dukes of Mantua and the Venetian Republic. Such was the thrill of the hunt in the Ferrara archives that Zaghi continued to do research and gradually moved to the period of papal domination of the region and finally to the *triennio* (1796–99). Like other historians of his generation and training, he began with a study of politics, especially of the movement for constitutional government in the preunification Italian states, and produced biographical profiles of major Jacobin leaders. After the war, however, he shared in the revival of scholarly interest in social and cultural history. In his beloved archives he searched for clues about the social structure of late eighteenth-century Italy, the position and influence of the clergy and the nobility, the role of Francophile intellectuals, and the attitudes of the European powers toward the Jacobin republics.

A massive anthology of chapters, essays, and sketches published over many years reflects the slow maturation of the author and the ever-increasing breadth of his interests and knowledge. The volume is organized chronologically to cover the period from the 1780s to the founding of the Napoleonic Kingdom of Italy. Within this chronological framework the material is arranged by topic. A bibliography of Zaghi's scholarly and journalistic writings, compiled by his former student Pasquale Sarli, completes the work.

The book confirms Zaghi's reputation as one of the most prolific scholars of the Jacobin and Napoleonic periods. More than that, this volume highlights the pioneering nature of Zaghi's work, on which scholars in Italy and other countries have built. Thus, for instance, his analysis of social structure in northern Italy before the French revolution (chapter 1) may appear sketchy, even naive, compared with more recent works in the field. It lacks the methodological and theoretical sophistication of social history as practiced by the heirs to the Annales school or, in the English-speaking world, by Peter Laslett and Lawrence Stone. But Zaghi's insights are the fruit of decades of painstaking reconstruction of available historical sources that he opened up for others, making possible both descriptive and interpretive work. This volume is not only a monument to the industry and learning of one man but also a very important contribution to recent Italian historiography. Zaghi's own evolution from enthusiastic but not very discriminating pursuer of archival "finds" to careful student of the broad social, cultural, and political trends of the late eighteenth century exemplifies the impressive intellectual development of historical scholarship in his country, especially in the postwar period. For that reason, even readers who are not particularly interested in the events and people of the *triennio* will find this volume useful, and they will be reassured, I believe, that historical scholarship is alive and well and (sometimes) fun to read.

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CARLO ZAGHI. *L'Italia di Napoleone dalla Cisalpina al Regno.* (Storia d'Italia, number 18, part 1.) Turin: Unione Tipografico-Editrice Torinese. 1986. Pp. xiv, 813.

Since the nineteenth century, when Italy finally achieved national unity, its historians have debated the impact of French revolutionary ideas and of Napoleon's rule in the peninsula on the formation of the nationalism that led to Risorgimento. Did French hegemony from 1796 to 1814 represent the starting point of Italy's national awakening, or had it merely substituted one foreign domination for another? Was not a burgeoning self-awareness already emerging during the eighteenth century? The debate reached frenzied heights under Fascist rule. French influence was denounced as another foreign intrusion in Italy's historical evolution as a nation, and the autochthonous inspiration for Italian nationalism was found in all of Italian history before 1789. A more balanced view has been emerging in the historiography of the post–World War II period.

After more than fifty years of research and writing on the Napoleonic period in Italy, Carlo Zaghi has contributed a magisterial study of its evolution in northern Italy from 1796 to 1814. A perceptive and detailed study of Napoleon, his policies, goals, and character, the book argues that Italians first acquired a sense of national identification under Napoleon, whether they worked for him or conspired against him. To be sure, Napoleon was a despot. He crushed freedom, despoiled Italy's artistic patrimony, and subordinated Italian interests to those of a greater France. Yet, "unwittingly, more than any other man . . . [Napoleon] opened the way

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for the Risorgimento" with the changes his regime effected in the social structure of the country. By awakening forces long dormant he "taught Italians to look beyond narrow municipal confines" and to become more aware of Italy as a whole and of Europe (p. 678).

Although France refashioned political, social, and economic institutions throughout continental Italy, its influence weighed more heavily in the northern half of the country, where a new unity was imposed. By French fiat and with dizzying rapidity, the Cispadane republic of 1796 gave way to the Cisalpine in 1797. Terminated in 1799 by the retreat of the French from Italy and the return of Austria with the support of its Russian allies, it reemerged as the second Cisalpine republic after Napoleon's triumphal reentry into Milan on 17 June 1800. Two years later it was replaced by the Italian Republic and finally by the Kingdom of Italy, which endured until Napoleon's final defeat. With these changes in political designations came fundamental and radical reforms. Not only were the temporal power of the church abolished and the basis of a secular society laid but the person and authority of the pope himself were challenged. Traditional boundaries disappeared, as territories were shifted around to serve French interests. Historical differences and rivalries were temporarily forgotten. The Napoleonic Codes and uniform weights and measures and administrative procedures replaced the ancient hodgepodge. An army fighting under its own officers and flag provided Italians with a military identity.

According to Zaghi, these changes brought forth "a new generation of Italians . . . free of regional, political, social, cultural, and racial prejudices, more alive and aware than preceding and even later generations" (p. 679). For the first time in their history Italians shared a sense of their own identity and developed common national bonds. Even those Italians who opposed Napoleon did so in the name of Italian freedom and independence, rather than in defense of local privileges.

In tracing the establishment and governance of the Kingdom of Italy under the regency of Eugène de Beauharnais, Zaghi has also analyzed the impact of Napoleon's rule on the entire peninsula. Although the history of southern Italy for the same years will undoubtedly constitute a separate volume in this new UTET history of Italy, under the editorship of Giuseppe Galasso, the principles and goals that inspired Napoleon's policies in Italy and their consequences for its people are clearly set forth by Zaghi.

The book is organized in twelve chapters, plus an introduction and a conclusion. A magnificent critical bibliography of primary sources and secondary studies (pp. 683–791) contributes to the scholarly importance of the volume. Reflecting the new trends in historical research and writing, Zaghi discusses not only political and military history but also social and economic changes, which emerge as the most important results of these years. Enriched by numerous illustrations and an index of names, this volume is an example of the best contemporary Italian historical scholarship, both in content and format. If a cavil may be allowed, maps showing the shifting territorial boundaries imposed by Napoleon on his Italian domains would have been helpful, but this is a minor shortcoming in a work so rich in scholarly insights.

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ISTITUTO PER LA SCIENZA DELL'AMMINISTRAZIONE PUBBLICA. *L'amministrazione nella storia moderna.* In two volumes. (Archivio, new series, number 3.) Milan: Giuffrè. 1985. Pp. xxxvii, 1088; xi, 2364. L. 160,000.

In his introduction to one of the sections in this collection of articles Piero Aimo concedes the inherent difficulty of history as anthology: at best the individual analyses can only in part correspond to the methodological objectives "in wide-ranging cultural undertakings of this type" (p. 553). Indeed, some thirty specialists in Italian history cannot be expected to produce a coherent interpretation of how administrative institutions in the varied regions of Italy took shape, from the age of Maria Theresa to that of Giolitti. Instead, they give us specialized reports based on their own archival research. English summaries of the articles in part 1, "Administration in Modern Italy"—the vast bulk of the book—are provided, but these amount to little more than abstracts of the long and densely packed arguments that will make heavy demands on the reader's time and concentration.

Although lacking a systematic account of its subject, the study possesses a consistent theme: politics begins in rhetoric and ends in bureaucracy. Section 1 of the first part, consisting of seven essays, reinforces the argument of Franco Venturi in *Settecento riformatore*, concerning the centrality of Enlightenment idealism in modern Italian political culture. Politically subjugated and economically stagnant, the Italians lagged far behind their powerful neighbors to the north in the development of modern administrative institutions. The Old Regime did not concern itself with establishing channels of official power leading from the government to every sector of society—especially the economy, public health and hygiene, education, postal service, water rights, and conservation—to nearly the same extent that the states of the future would.

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The main strength of the first section lies in the examples—in Genoa, Bergamo, Venice, Milan, and Pavia—the authors have provided of the peninsula's backwardness and of its halting, imitative attempts to reform. Inspiration to move forward initially came from the Austrians, whose presence in Milan made that city the focal point of Italian administrative modernization, although even there the reform movement was neither homogeneous nor linear. The *illuministi* condemned what they viewed as the outmoded institutions of the Old Regime, by then overgrown with class, professional, and territorial entitlements. They intended to rationalize the exercise of administrative power in the central government by systematically eliminating competing units of authority.

Nowhere in Italy were the enlightened ideals of the reformers fully realized, and only the intervention of Napoleon ensured Italian administrative modernization. In effect, Napoleon appeared as the long-heralded prince of the Enlightenment, and his model of administrative monarchy originated in the ambitious but only partly realized political theories of enlightened absolutism. He envisaged the administrative apparatus as the means for enlightened regeneration. Accordingly, the Napoleonic state set hitherto unapproached standards for bureaucratic intrusion in the lives of ordinary citizens.

The second section of part 1 contains six accounts of Italy's first major experience with modern state administration. As in the first section, the overall design of these chapters is not systematic; each one stands on its own, yet the cumulative impression is uniform. Statistical surveys began to pile up as the government sought to learn more about society, the better to change it in accordance with bureaucratic agenda, tax it more profitably, and conscript soldiers more efficiently. The Kingdom of Italy received a statistical office, a gift from the mother bureaucracy in Paris. Not surprisingly, its surveys helped spread the belief, essential to the growth of bureaucracy, that without the information provided by their indefatigable authors the vitality of government and of civilization itself would be sapped. The movement of governmental paper possessed a recognizably modern shape in the Napoleonic empire. One of the best essays in the section, on Gian Domenico Romagnosi, meticulously depicts how the author of *Principi fondamentali del diritto amministrativo* (1814) celebrated the achievements of Napoleon's bureaucratic revolution and why this celebration helped establish the sanctity of authoritarian paternalism in the country's study and practice of public administration.

Obsessed by the vision of a monolithic empire, Napoleon ignored regional differences in Italy. Regulations for Milan were also imposed on Naples, with dismaying results. The Italian nation-state repeated this mistake later in the century, and the thousand-page third section of part 1 considers the difficult post-Risorgimento period during which a large part of the population viewed the national government as an oppressive power. The administrative reforms of Francesco Crispi in 1888 further centralized the Italian government, until then based on the original French-Piedmontese administrative model of 1859. More than ever before Rome viewed local administrations as only the agents of its will. Vittorio Emanuele Orlando lent his enormous prestige as a scholar to the campaign for "vigorous centralization," on the grounds that Italy lacked the political class necessary for self-government. Throughout the period from 1890 to 1915 state jurisdiction grew at the expense of communal government.

Local governments received a vital new impulse from the movement known as *municipalizzazione*. They rapidly took over such city services as communications, transportation, gas, electricity, education, and public health. Deprived of political power by the centralizing process, local administrators nevertheless greatly assisted the bureaucratization of the Italian economy as first the practice and then the theory of classical liberalism fell into disuse. Under the pressure of breakneck urbanization, municipal functions, too, had to be rationalized, and this meant a sharp curtailment of unrestricted business practices in sectors of the economy deemed essential to the public good. Thus, the "social vocation" of the commune became legitimized under the auspices of the state's higher authority, and a law of 1903 rendered this relationship fully explicit.

What Camillo di Cavour had begun and Crispi had augmented Giovanni Giolitti completed: the erection of a tightly centralized state administration. The thirteen essays in section 3 provide a wealth of detail about that lengthy and, ultimately, unsuccessful development. Fascism undid the work of Italy's liberal government. With only a few exceptions, notably the essays on the chambers of commerce and the National Rice Corporation, the authors do not take us beyond the Giolittian period. The glimpses we get of the Fascists show them to have been protagonists of radical centralization. However, far from carrying forward the administrative revolution of the liberal state, they first perverted it and then obliterated it by creating a government so different in degree as to be different in kind. This is obviously not material for the general reader, who in section 3 would find excessive detail about the rivalries and office politics in various government ministries, but here the specialist in modern Italian history will find invaluable statistical and biographical information about the personalities, ideas, and practices that shaped the country's administrative structure.

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The book closes with a two-hundred-page part 2, made up of three essays on the history of administration in France, Germany and Austria, and Spain. These are much more general in scope than the articles in part 1, but the contrast they afford highlights the implicit thesis of the book, that the history of Italy's political administration took the course it did as a result of the country's sequential subservience to Austria, France, and Piedmont.

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LUIGI DE MATTEO. Holdings *e sviluppo industriale nel Mezzogiorno: Il caso della Società Industriale Partenopea, 1833–1879*. (Ricerche di storia economica, number 3.) Naples: Istituto Italiano per gli Studi Filosofici. 1984. Pp. 138.

In 1833 and 1834 the protectionist policies of the new monarch, Ferdinand II, stimulated an investment boom in the Kingdom of Naples. The number of joint-stock companies doubled in Naples. These new finanacial companies were interested in speculative, short-term investments that yielded high returns. They contributed, however, to the revival of the sugar, textile, glass, and leather industries in Naples.

Luigi De Matteo has studied the history of one of the most successful of these companies, the Società Industriale Partenopea—from its birth in 1833 to its demise in 1879. In its early years the Partenopea was a financial holding company involved in discounting and lending operations, in credit activities, and in the dye-stuff and printing materials industry. In 1834 it extended its activities to the manufacture and sale of woolen carpets, glue, silk textiles, sugar, and porcelain. In 1837, after an abortive attempt to establish a large-scale plant to produce cotton yarn with John Cockerill, a Belgian industrialist, it built a mechanical spinning mill at Sarno for the manufacture of linen yarn and textiles. The decision to produce linen rather than cotton largely resulted from competition in Salerno from the Swiss cotton firms that had set up in southern Italy to avoid the effects of the Bourbon tariff.

The Partenopea flourished through the 1840s and 1850s, becoming the largest manufacturer of linen textiles in southern Italy. Under an able and efficient manager, Augusto Sideri, it yielded a respectable return to its stockholders. The industry, however, relied heavily on tariff protection and depended exclusively on the internal market for consumers. Because of its low level of technology and limited diversification of products, it was unable to compete with English and French linens in the international market.

In the decades after unification, particularly with the recovery of cheap cotton textiles after the U.S. Civil War, the linen industry encountered difficulty. The Partenopea was unable to respond to the challenge. Faced with heavy indebtedness, needing fresh infusions of capital, lacking proper entrepreneurial guidance, and confronted with new free-trade treaties, falling prices, and growing competition from more efficient producers (in northern Italy and Europe), the Partenopea collapsed. Parenthetically, at this time Swiss cotton manufacturers (Schlaefer, Wenner, Vonwiller, and so on) in Salerno (the Manchester of the south) had begun a highly successful reorganization of the cotton industry. The mentality of the Swiss industrialists was quite different from that of the Italian entrepreneurs of the time.

Agriculture, however, not commerce, was the main business of southern Italy in the nineteenth century. Investment was attracted to more traditional branches of economic activity, and wealth and power depended on monopolistic privilege conferred by the state. Economic power rested not on trade but on capital and credit—particularly on credit to the state. This has been well documented in John Davis's recent work, *Merchants, Monopolists, and Contractors in Bourbon Naples* (1981).

De Matteo nevertheless has skillfully analyzed the financial, economic, and administrative activities of the Partenopea. Not the least of his contributions has been to relate the complex interconnections of his story to the broader historical framework.

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FILIPPO SPATAFORA. *Il comitato d'azione di Roma dal 1862 al 1867: Memorie*. Volume 2. Edited by ANNA MARIA ISASTIA. (Domus Mazziniana, Pisa; Collana Scientifica, number 17.) Pisa: Nistri-Lischi. 1984. Pp. 628–1224.

The pious tradition of publishing the correspondence of even minor patriots is one of the assets of research on the Risorgimento, and Filippo Spatafora's memoirs are in that tradition. Assembled a century ago as an old man's effort to set the record straight, more than half of the text consists of the letters he sent and received a generation earlier. Spatafora was a central figure in the Roman comitato d'azione, and his memoirs and correspondence provide a detailed account of the committee's efforts during the 1860s to build within papal Rome a revolutionary base that would both complete Italian unity and transform the moderate Italian kingdom into a more Mazzinian polity. The first volume covered 1862–63, and this one deals (unevenly) with

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the next four years in more than five hundred pages of fine print.

Much of the correspondence was with Giuseppe Mazzini and the Tuscan radical, Giuseppe Dolfi: as these names suggest, those associated with the committee were more or less republican. Despite a certain vagueness and many disagreements, they shared a Mazzinian dream of Italy's regeneration (even when keeping their distance from Mazzini) and were fervent democrats. Relentlessly serious, they explored every event to determine the stand they should take, worried over the comments of every newspaper, and wrote wordy appeals to the Roman people, alternately urging them to boycott festivities in honor of Pius IX and to undertake some "virile action."

Theirs were the busy and self-important lives of conspirators, signing their *noms de guerres* to coded messages that were not always delivered or fully understood and furiously analyzing every move of their competitors in the moderate comitato nazionale. This was serious business; many of their number were imprisoned, and some died in jail. Spatafora provides statements of the (pathetically small) sums that they received from abroad and meticulous accounts of how that money was spent to aid prisoners, buy arms, and print posters. Awash in intrigue, Spatafora was at home with the involute politics of the papacy. He had sources in the bureaucracy with whom he could discuss in detail the corruption of Cardinal Antonelli and the latest news about the pope's health. Speculation about the import of the government's most minor moves was taken as evidence of a well-informed party ready for any event. Like the preoccupation with Pius IX's imminent death, it instead reveals bankruptcy.

These figures who had once imagined Rome at the center of world history now felt that the pressures from outside—the policies of the Italian government, the power of Napoleon III, the influence of diplomacy, or Roman moderates in exile—could only frustrate their cause. Too deeply engaged to abandon their efforts, they did not have a sense of history that could give them much hope. They candidly noted that the moderates were gaining and that the republicanism of some of their own followers was suspect, but they could only ascribe these developments to individual weakness and sinister plots. By 1866 they were lamenting the growing influence of Mikhail Bakunin and his "nordic theories" in Naples. These moral men wrote with power about the corrupt incompetence of papal bureaucracy and the social insensitivity of the moderates. They proudly insisted that the Roman people were "neither corrupt nor superstitious" but merely lacked political education. So did the men of the comitato d'azione. Interesting for what they say and for the minor biographies they provide, these documents are most revealing in their silences. Spatafora says nothing about mobilizing mass support or postrevolutionary political programs. Even when he assembled these memoirs, he was primarily concerned to show his personal rectitude.

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DAVID FORGACS, editor. *Rethinking Italian Fascism: Capitalism, Populism, and Culture*. London: Lawrence and Wishart; distributed by Humanities, Atlantic Highlands, N.J. 1986. Pp. xxi, 209. $29.95.

This book attempts a revision of current thinking on Italian Fascism that goes beyond historical questions to include a consideration of such cultural problems as cinema and literature. Seven of the essays were originally presented as public lectures in Great Britain in 1984, and two others, plus the editor's introduction, were written especially for this collection. The lectures were presented for a nonspecialist audience and retain this accessible character in print.

The volume clearly represents the position of leftist British academics or leftist Italian academics who hold positions in Great Britain on Italian Fascism. Consequently, the canonical views of Antonio Gramsci or of Nicos Poulantzas (a Greek Marxist theorist and author of *Fascisme et Dictature* in 1968) seem to inform many of the most overtly ideological essays in the volume. As is so often the case, the more concrete and specific essays are the most useful, whereas those that attempt in only a few pages to present a reevaluation of Marxist theory on Italian fascism will leave the reader less satisfied. Geoffrey Nowell-Smith's "The Italian Cinema under Fascism" and Lino Pertile's "Fascism and Literature," for instance, are first-rate surveys that avoid polemical positions and would be rewarding reading for both students and scholars. Tobias Abse's "The Rise of Fascism in an Industrial City: The Case of Livorno, 1918–1922" and Jonathan Steinberg's "Fascism in the Italian South: The Case of Calabria" are equally interesting case studies based on extensive archival research; they juxtapose two diametrically different experiences and underline the difficulty of making generalizations about Italian Fascism. Paul Corner's "Liberalism, Pre-Fascism, Fascism" is another useful consideration of the transition between Italian liberalism and Fascism. Other essays analyze Marxist thinking on Fascism (David Forgacs), Fascist policy on women and the family (Lesley Caldwell), oral memory of the Fascist period among working-class Italians of Turin (Luisa Passerini), and the revisionist positions taken in the anthology itself (Roderick Kedward).

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Although the editor obviously hopes this book will be adopted in classrooms, and a need for a book that could be used by a number of disciplines seems obvious, this volume should be given to American undergraduates with some caution. In the essays I have noted for their special interest, the obvious ideological slant typical of some of the contributions is absent. At least one of the motivations for this revision of perspectives on Mussolini's Italy is the expressed desire of some of the contributors to apply a revised definition of "Fascism" to liberal regimes that are not to their liking (especially Margaret Thatcher's government). In this sense, this collection inadvertently tells an American reader something interesting about the political views of British academics.

In addition to the essays, the volume contains some very useful material for the student: a chronology of the major events in Italian history during the *ventennio*, a handy list of abbreviations associated with the Fascist regime, and a glossary of Italian terms employed in discussions of Fascism.

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ŽIVAN MILISAVAC. *Istorija Matice srpske.* Volume 1, *Vreme nacionalnog budjenja i kulturnog preporoda 1826–1864* [History of the Serbian Matica. Volume 1, The Period of National Awakening and Cultural Rebirth, 1826–64]. Novi Sad: Matica Srpska. 1986. Pp. 974.

The oldest continuously operating literary and cultural society in Southeastern Europe is Matica srpska, a title that can only awkwardly be rendered into English as, perhaps, the Serbian Matrix. Živan Milisavac's nine-hundred-page monograph, which is the product of a lifetime of research and writing, describes the first forty years of the society in mind-numbing and fascinating detail—mind-numbing because he mentions every person who applied for a scholarship, submitted an article, or attended a meeting of the society but fascinating because his patience and love of detail provide unique descriptions of the cultural and social history of Serbs in the Austrian empire during these transitional years.

Matica srpska was founded in Pest by seven middle-ranking Serbian merchants to support the publication of a new journal introduced two years earlier in Novi Sad. But during the period of its ensuing publication in Pest, neither this journal, *Letopis*, nor the series of monographs that was the society's second major cultural endeavor attained the influence or quality their founders hoped for. One reason was the underdeveloped state of Serbian literature, but another was that Matica ignored the handful of authors who were producing high-quality work. Concentrating on a series of futile literary and grammatical competitions, it lost the support of the most important scholars of the day, such as Pavel Šafárik, and its middle-of-the-road position on issues of language and orthography prevented cooperation with Serbia's most able linguist, Vuk Stefanović Karadžić.

The most important aspect of the early history of the society was not literary at all, but the participation of Sava Tekelija, a wealthy Serbian nobleman who, because he had no family except an estranged wife, took an interest in the society in his old age and eventually bequeathed it his considerable fortune. Tekelija's money, along with sizeable gifts from others, made Matica srpska and its associated foundations the richest private foundation in the Habsburg empire. In the long run Tekelija's bequest assured the future of the society, but in the short run it undermined its purpose. Meetings of the society became devoted to the minutiae of financial management, which the mercantile and legally trained membership understood and appreciated, and Matica turned into more a lending institution than a cultural one. The increased respectability of the organization attracted conservative members who were unwilling to permit editorial changes that might improve the dismal performance of *Letopis* (its circulation never reached 250). The society also became bogged down in an interminable series of litigations with Tekelija's estranged wife. When the changed political situation in Hungary at the end of the Bach regime permitted the emergence of Novi Sad as the cultural and political center of Serbs in Hungary, the increasingly isolated leadership in Pest finally agreed, albeit in a laconic and disorganized way, to move the society to Novi Sad, where it has prospered ever since.

Milisavac's description of the lawsuits over Tekelija's fortune well illustrate the social constraints of the Habsburg empire, as Matica struggled to find sufficiently distinguished individuals to back its cause. The description of the institution Tekelija founded to aid impoverished Serbian students (the Tekelijanum) reminds us of the serious problems of health and sanitation common to the era. Of 105 needy students who received these scholarships, 12 died during the term of their stipend. By the late 1850s the health of those living in the Tekelijanum was so bad that a doctor was visiting the institution every day. Milisavac's mention of the educational and social background of most of the people he names not only shows that legal training was the main route to social mobility for Serbs by the 1840s but also demonstrates that before 1848 the Habsburg empire contained a number of rich and even noble Serbs, most of whom, however, were thoroughly Magyarized.

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In short, this massive book is both a compendium of data and the definitive work on its rather narrow subject. At a time when the historical profession is being swept by theoretical winds of great variety, it is not unsatisfying to settle down for a long read with an old-fashioned magnum opus.

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JAN KOZIK. *The Ukrainian National Movement in Galicia, 1815–1849*. Translated by LAWRENCE D. ORTON and ANDREW GORSKI. Edited by LAWRENCE D. ORTON. Edmonton: Canadian Institute of Ukrainian Studies, University of Alberta. 1986. Pp. xx, 488.

The development of Ukrainian national consciousness in Galicia in the two decades before the revolution of 1848 and the attempt to translate ideals into political institutions during the springtime of peoples follow a path familiar to historians of other national movements in the Habsburg Monarchy. Jan Kozik treats the Ukrainian pattern from within and consequently emphasizes the writings of its participants as the primary sources. The result is an immensely detailed work, which, although it brings out the unique characteristics of the Ukrainian movement, never loses sight of its broader, European connections.

In a sense, Kozik's work is a collective portrait of the Ukrainian generation of 1848 as the bearers of Ukrainian national consciousness. As elsewhere in Eastern Europe, they were intellectuals, and, because Ukrainian society was overwhelmingly rural, they were also clergymen. By training and temperament they put great store by education—"enlightenment," they called it—and were certain that if ignorance could be overcome beneficial social change would follow in due course. The building of schools and the writing of textbooks were for them civic duties, but questions of higher culture aroused their enthusiasm. The work of the so-called Ruthenian Triad—Markiian Shashkevych, Ivan Vahylevych, and Iakiv Holovatsky—who led the national movement with their studies of the Ukrainian language and history in Galicia and their attempts to found a modern literature, are reminiscent of the activities of Slovak and Romanian intellectuals of Hungary at a somewhat earlier period.

The controversy over the literary language and the alphabet, in which the triad was deeply involved, was a typical intellectual preoccupation of the time. Kozik keeps the many strands of this complicated issue from becoming entangled and judiciously uses the resulting data to measure the level of Ukrainian national consciousness. Strikingly, in the 1830s the polemics were carried on mainly in Polish, evidence of the enormous influence of Polish literature and language among Ukrainian intellectuals and the relatively limited development of Ukrainian. Yet, just as important in the 1840s was the anti-Polish current, which intensified under the impress of the romantic notion that language was the clearest expression of nationality and that its loss signaled the disintegration of the nation itself. The concomitant debate over which alphabet, Latin or Cyrillic, was proper for the new literary language went beyond linguistic problems. Those who objected to the Latin alphabet argued that its use would strengthen ties with the West and divide the Ukrainians of Galicia from their conationals under Russian rule and other Slavs. Consistently hostile to the West, they held its institutions, notably feudalism, responsible for the disruption and disintegration of medieval Slavdom.

Kozik treats 1848 as a year of crisis for the Ukrainian national movement, as one of difficult choices and rapid political maturing. He describes at length the programs and the organizations of Ukrainian intellectuals, the divisions within their ranks between conservatives and liberals, and the role of the clergy and peasants. But he makes clear that events in Galicia were dominated by the insurmountable rivalry between Ukrainians and Poles. Their unwillingness to compromise as they strove to achieve national ambitions led to one of the characteristic tragedies of 1848 in Eastern Europe. Ukrainians and Poles who shared a common concern for civil and political liberties found themselves at odds over national rights. Liberal (and conservative) Poles offered the Ukrainians equality, but only as individuals in a Polish Galicia or in a reconstituted independent Poland. Liberal (and conservative) Ukrainians, who were anxious to preserve their own language and nationality, could not accept these terms. The beneficiary of this conflict was antiliberal and antinational Austria. The story is strikingly reminiscent of the struggles between the Magyars and the Slavs and Romanians in Hungary.

This book, a translation of two monographs that appeared in Polish in 1973 and 1975, is a singular contribution to the scholarly literature on Ukrainian nationalism in English. It deserves to be read also by those interested in the comparative study of modern national consciousness.

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MICHAEL WORBS. *Nervenkunst: Literatur und Psychoanalyse im Wien der Jahrhundertwende*. Frankfurt a/M.: Europäische Verlagsanstalt. 1983. Pp. 383. DM 48.

JACQUES LE RIDER. *Le cas Otto Weininger: Racines de l'antiféminisme et de l'antisémitisme*. (Perspectives critiques.) Paris: Presses Universitaires de France. 1982. Pp. 255. 100 fr.

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Vienna has justly come to be regarded as a historic location for thinking and writing about sexuality, arguably the central theme of Viennese modernism in the generation or two after 1890. The books under review explore this thinking about sexuality from the perspectives of two psychologists who became famous in Vienna during the first decade of the century: Sigmund Freud and Otto Weininger. Both books draw on recent work by American historians (particularly Carl Schorske and William McGrath) to develop arguments about the context of Viennese culture and politics that go beyond conventional formulations about eroticism and morbidity, but they also emphasize the wider European context of psychiatry and *belles lettres*. Michael Worbs explores the connections between psychoanalysis and Austrian literature, and Jacques Le Rider recalls that much of this thinking about sexuality amounted to men thinking about women.

Nervenkunst: Literatur und Psychoanalyse im Wien der Jahrhundertwende is an informative literary-historical study, based on Worbs's dissertation. Worbs is primarily interested in the reception of Freud's thought by *Jung Wien*, and his main sections describe Freud's relationship to Karl Kraus, Arthur Schnitzler, and Hugo von Hofmannsthal. Worbs wants to show that Freud and *Jung Wien* (to which Schnitzler and Hofmannsthal, but not Kraus, belonged) shared common intellectual and cultural ground. Although Freud had only minor personal and intellectual connections with literary Vienna, Worbs argues that the concerns of nineteenth-century psychiatry and Austrian literature converged in an introspective *Nervenkunst*; both psychoanalysis and *Jung Wien* developed psychological, inward visions out of the naturalism and positivism of the late nineteenth century.

Much of the information will be familiar to those who work in the field, but Worbs's thorough research offers interesting juxtapositions, especially regarding Schnitzler and Hofmannsthal. Worbs argues that the differences between Freud and Kraus have been exaggerated because of the personal role of Fritz Wittels in mediating the exchange of ideas between the two men. Certainly, Freud and Kraus were both committed to sexual enlightenment, but they differed in their views on the relationship between sexuality and creativity. The real similarities, however, are between Schnitzler and Freud. Freud saw Schnitzler as his double, and Schnitzler was far more interested than the other poets of *Jung Wien* in psychoanalysis. Like Freud, Schnitzler emphasized the power of sexuality and the yearning for union that is often a threat to social convention and to the security of the ego. But Schnitzler's notes after 1921 show that he considered Freud too speculative and systematic, and the actual influence of Freud's thought on his work was slight. Hofmannsthal shared Freud's impulse to psychologize classical myths, and Worbs emphasizes the similarity in their reception of the ancients. But Freud and Hofmannsthal lived in entirely different social and intellectual worlds, and Hofmannsthal regarded Freud as "an absolute mediocrity" (p. 267). Freud, for his part, had the wisdom not to expect dramatists to portray his theories on the stage; he agreed with Robert Musil that, rightly understood, there is no such thing as a psychological literature. The yield of these juxtapositions is modest, and Worbs does not provide his long book with a conclusion. But his central argument seems to be that Freud's work inadvertently matched the mood of *fin-de-siècle* literature; Freud liked naturalism and avoided reading the moderns, but he helped overcome the naturalism he admired.

Otto Weininger (1880–1903) had no use for *Jung Wien*, and Freud saw little promise in the young man's dissertation. Weininger was close to Kraus in his use of the theme of sexuality to condemn the culture of liberal Vienna, and he was perhaps the most radical critic of conventional notions of sexuality and gender at the turn of the century. Le Rider argues that Weininger was actually more famous than Freud in the first decade of the century, and feminists will be surprised to learn that Weininger regarded psychoanalysis (like science and capitalism) as a feminization. *Le Cas Otto Weininger: Racines de l'antiféminisme et de l'antisémitisme* reflects recent French interest in Austrian history and literature and the wider European revival of interest in Weininger in particular; it is now the best book on Weininger, and Le Rider effectively sets Weininger in the intellectual context of his time.

As Le Rider emphasizes, Weininger formulated his critique of culture as an attack on femininity and the Jew (he was himself Jewish but converted to Protestantism). Le Rider is kinder to Weininger than most critics, but he is still reluctant to take seriously Weininger's distinction between the feminine principle (*das Weib*) and individual women (*die Frau*), and Le Rider cannot always resist the temptation to equate antifemininity with antifeminism. But to see Weininger's critique of sexuality (which he regarded as feminine) simply as conventional male opposition to feminism or women is not entirely fair. At the very least Weininger belongs (like Schopenhauer and Nietzsche) to the honorable misogynists of a profoundly sexist high culture. But his radical argument on behalf of human freedom is almost breathtaking in its opposition to conventional social roles and the traditional manipulation of women as sexual objects and mothers. Weininger preferred to see the human race die out (here he meant to be more consistent than St. Augustine) than to postpone human freedom for women. He suffered so terribly under these ideas that he took

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his own life at the age of twenty-three, only a few months after publishing *Geschlecht und Charakter*. Le Rider faults him for confusing literature and reality and emphasizes the pathos of this earnest young man, but Weininger also deserves respect for demanding an immediate end to human slavery.

Le Rider emphasizes the links between Weininger's misogyny and his anti-Semitism and the connection of this way of thinking to political reaction. But he also links Weininger's ideas to nineteenth-century philosophy, literature, and science, and he argues that Weininger should not be regarded as marginal or idiosyncratic, given the strong (and often sympathetic) responses of his contemporaries. Perhaps the most interesting aspect of Le Rider's account is his presentation of Weininger in the context of modernist doubts about romantic union and sexuality. Le Rider argues that Weininger was "one of the first to emphasize the great theme of modernity: existential solitude" (p. 239). It is not entirely clear, to be sure, that modern existential isolation has liberated itself from the romantic dialectic of yearning and despair, since modernist isolation may be only a waste product of the original romanticism.

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JOHN D. BELL. *The Bulgarian Communist Party from Blagoev to Zhivkov*. (Histories of the Ruling Communist Parties.) Stanford, Calif.: Hoover Institution Press. 1986. Pp. xii, 202. $9.95.

This is the tenth volume to be published to date in the series, "Histories of Ruling Communist Parties," edited by Richard F. Staar. The nine earlier volumes include four on ruling Asian Communist parties (those of North Korea, Vietnam, Mongolia, and Afghanistan), four on East European Communist parties (those of Poland, Hungary, Czechoslovakia, and Romania), and one on the pivotal and hegemonial Soviet Communist party.

Bulgaria is the only so-called socialist state of Eastern Europe that has never experienced a crisis in its relations with the Soviet Union since the end of World War II. In return for its sustained loyalty and docility, Bulgaria receives substantial Soviet economic support, inflated prestige within the international Communist movement, and military protection. This is important for a small country that lost three consecutive wars in the twentieth century (the Second Balkan War of 1913 as well as both world wars) and whose territorial integrity is even now not fully acknowledged by its largest neighbor, Yugoslavia. Thus, although it may not be heroic, Bulgaria's accommodating stance toward the Soviet Union appears to be rational and is certainly prudent.

Having established his solid professional reputation in 1977 with *Peasants in Power: Alexander Stamboliski and the Bulgarian Agrarian National Union, 1899–1923*, John D. Bell has now written a book of a quite different and in some ways more difficult genre. The volume under review is a survey of a century of socialist and then Communist political life in Bulgaria (and of governmental power since 1944) condensed into 147 pages of text and 10 pages of appendixes. The distillation required for such a condensed study is surely more difficult than a comprehensive treatment that, say, 400 pages would have permitted. Inevitably, other specialists on Bulgaria or on comparative communism will regret that some of their favorite episodes, characters, or lessons have been truncated or omitted. But such omissions are inevitable in this type of survey—and students will appreciate its incisive brevity. The book is objective, thorough, reliable, and useful. Only in the last half of the last chapter, in which he approaches the present moment, does Bell occasionally (and understandably) have difficulties in maintaining sufficient distance and perspective to distinguish between solid trends and ephemeral deviations.

Three small errors should be corrected in any future reprinting of this productive book: in September 1953 Bulgaria marked the ninth (not the eighth) anniversary of the Communist seizure of power in 1944 (p. 112); in 1958 the country marked the thirty-fifth (not the fiftieth) anniversary of the Communist September Uprising of 1923 (p. 118); Todor Zhivkov has not held power longer than any other Soviet-bloc leader, being surpassed by North Korea's Kim Il Sung (and, in their days, by Yugoslavia's Tito and Albania's Enver Hoxha, although Bell might counter that their respective states had ceased to belong to the Soviet bloc in 1948 and 1968 [p. 125]).

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JANOS BERECZ. *1956: Counter-Revolution in Hungary; Words and Weapons*. Budapest: Akademiai Kiado. 1986. Pp. 223. 160 Ft.

This thin volume is an English-language reissue of a book published in Hungarian by Janos Berecz ten years ago. Its major themes are that the revolution in 1956 was really a counterrevolution, that it was fomented by imperialist machinations and aimed at restoring "feudal-capitalism," and that only the "fraternal assistance" of the USSR saved the true revolutionaries—that is, Janos Kadar and his allies in the Hungarian Communist party. The study, if one can

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call it that, was received with disdain and disgust by a large number of people on its original publication; it has not been improved by the competent English translation. Printed so that it will be available for the thirtieth anniversary of the crushed revolution, this diatribe lacks both factual exactitude and decent humility before one of the great tragedies of Hungarian history. It is full of sarcasm such as, for example, the statement, "One report on the newly established Fliers Association and the passionate determination of its members to fly in a 'free country, on free wings and in a free air' must have brought tears of pride to many of its readers" (p. 146). His assertion on the same page that irresponsible elements began the mass arrest and persecution of "people loyal to socialism" implies a widely practiced terror against loyal Communists and ignores the remarkable infrequency of acts of violence against people loyal to Communism, given the hatred with which Stalinism and the Soviet Union were viewed. His figures showing that few people were even wounded by Soviet troops—and apparently none were killed—are contradicted by both East European and Western observers who estimated that at least two thousand people were killed. The documentation serves the documentor: propaganda meets its goal of providing untruths based on false data made to be hidden in "scholarly" guise.

The author is one of the more intelligent members of the younger generation of Hungarian Communist leaders. If his goal was to receive approval of Soviet progagandists for his hardline stance on an issue that even Kadar—the party's long-time general secretary and the man in charge of Hungary's destiny since 1956—calls a national tragedy, he has succeeded admirably. If he intended to demonstrate to the West that the revolution of 1956 was the result of Western machinations (chapters 2 and 3), he failed to convince anyone. And, if the eternal loyalists of the Hungarian Academy's respected publishing house intended this volume as an academically sound treatise on the subject, they have merely succeeded in denigrating their own efforts at convincing the West that objective scholarship is alive and well in Hungary. It is a pity that an intelligent man such as Berecz, one of those truly in contention to inherit the mantle of Kadar, stooped this low.

IVAN VOLGYES
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MANFRED HILDERMEIER. *Bürgertum und Stadt in Russland 1760–1870: Rechtliche Lage und soziale Struktur.* (Beiträge zur Geschichte Osteuropas, number 16.) Cologne: Böhlau. 1986. Pp. xix, 689.

To describe the legislation that governed the urban population of the Russian empire from 1760 to 1870 would constitute a major feat. So, too, would an analysis of patterns of social and economic evolution among the merchants (*kupechestvo*) and petty townspeople (*meshchanstvo*) in this period. Manfred Hildermeier has done both in a major study that portrays the complex relationship between the tsarist state and the urban social estates (*sosloviia*).

Hildermeier's work summarizes the findings of Western and Soviet researchers and presents a wealth of statistical and descriptive material drawn from laws and archives. Part 1 weighs the impact of legislation promulgated by Catherine II and her successors, and part 2 describes the guild reform of 1824 and the tax legislation of 1863–65, with special attention to the economic activities and mores of the urban population. No less than fifty-seven tables provide data on occupation, wealth, guild membership, and urban and rural location of industrial enterprises.

The great strength of this work lies in its clear depiction of the laws by which the tsarist regime regulated the urban estates, primarily to ensure the collection of revenues, even at the expense of commercial and industrial development. Despite half-hearted efforts to create a vigorous urban elite, the tsarist government refused to allow merchants to share the economic privileges of the gentry, primarily the ownership of land populated by serfs. The resulting flight of wealthy merchants into the gentry (*Standesflucht*) only weakened the merchant estate still more.

At the base of the social pyramid in the Russian city, the situation was even worse. Petty townspeople, unable to compete against serf artisans, lived mainly as servants, itinerant laborers, gardeners, and criminals: "a social estate without a defined function and without a social identity" (p. 464). Although the number of urban inhabitants more than doubled between 1719 and 1811, their share of the total population rose only slightly, from 3.6 to 3.8 percent, and even if the populous Polish lands annexed by Catherine were included only 4.4 percent of the tsar's subjects resided in cities in 1811 (p. 162).

Despite the abundance of factual detail presented here, the book raises some questions that researchers might well consider in the future. Hildermeier devotes much space to descriptions by foreign travelers. A clearer and more nuanced view from within is needed. Granted, few merchants could both read and write (35.9 percent in Saratov in 1844, according to table fifty-seven), and Hildermeier has gleaned much from the meager memoir literature. Still, works by such writers as Vladimir P. Riabushinskii, who left a remarkable treatise on the social and religious attitudes of his Old Believer forebears,

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might well have supplemented the observations penned by foreigners.

Second, as the author admits, his portrayal of urban life represents a distillation of reality based primarily on St. Petersburg, Moscow, and the major settlements of the central region, such as Vladimir and Nizhnii Novgorod. But what of Riga, Warsaw, Odessa, and other cities that did not fit this mold? The book somehow appears too short, despite its great length, to do justice to crucial regional variations.

Finally, Hildermeier calls the laws of 1863–65 "a de facto dissolution" of distinctions between urban estates (part 2, chap. 5), and certainly the granting of membership in the merchant estate to anyone who purchased a first- or second-guild certificate constituted a major departure from the traditional notion of hereditary occupational groupings. But the estate structure—essentially a set of social categories imposed on society by the autocratic state to promote its own interests—remained the core of tsarist social policy from 1649 onward and could not be so easily demolished. In some cities strong estate loyalties among the merchants persisted well into the twentieth century. Just as serfdom is the key to understanding the Russian city in the century before 1861, so the autocracy's determination to perpetuate the estate structure constitutes the primary fact of Russian social history from 1861 to 1917. It seems, therefore, an exaggeration to assert that the Municipal Reform of 1870, the educational incentives of the Military Reform of 1874, and the proliferation of banks, railroads, and joint-stock companies led to "a thorough change, especially in the upper ranks of the merchant estate, and to the emergence of an industrial 'bourgeoisie' of the modern type" (p. 599).

These observations are not intended to detract from Hildermeier's impressive scholarly achievement. American professors need only point to this massive volume to persuade graduate students in Russian history of the necessity of knowing German.

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M. A. ALPATOV. *Russkaia istoricheskaia mysl' i zapadnaia evropa (XVII–pervaia polovina XIX v.)* [Russian Historical Thought and Western Europe from the Eighteenth to the First Half of the Nineteenth Century]. Moscow: Nauka. 1985. Pp. 269. 1r. 70 k.

At the time of his death in 1980 M. A. Alpatov had published two volumes of his extended study entitled *Russian Historical Thought and Western Europe*, one dealing with the twelfth to the seventeenth century (1973) and one focusing on the seventeenth and first quarter of the eighteenth century (1976). The current monograph, although not a completed manuscript—it literally ends in midsentence—brings the story forward approximately to the middle of the nineteenth century.

To compose a review of a volume that is in such a manifestly unfinished state is not easy. One hesitates to be overly critical of certain obvious omissions—the book lacks an introduction and conclusion, and several of its sections appear to be little more than preliminary sketches that never were written up in full—simply because one does not know what Alpatov would have done if given more time. Nevertheless, the text is complete enough to allow at least a general assessment that is on balance rather mixed.

First, let me discuss the positive features. Alpatov clearly was well read in several subjects, including Russian and French thought, and he chose to place his narrative squarely in a European context. Frenchmen and Germans, whether from abroad or in residence in Russia, are seen in discourse with Russians, and vice versa. In its broad outlines, therefore, the book stands above the overly nationalistic style of writing intellectual history that dominated the Soviet historical establishment during much of Alpatov's professional life.

The book also contains a number of intelligently argued discussions of important figures who often suffer from underexposure in Soviet scholarship. The treatment of Petr Chaadaev stands out in this regard, as do the informative and generally judicious accounts of August Schlözer's historical views, M. F. Orlov's conceptions of society and economy, and the impressions of eighteenth-century French travelers in Russia.

But these highlights are too often obscured by the books more visible shortcomings. Most glaring is the lack of any apparent focus, thesis, or overall design. The choice of topics often seems to be random, and many of the issues that are mentioned are given insufficient attention. In part this problem arises from Alpatov's troublesome format, which consists of numerous short vignettes of several authors, be they historians, polemicists, memoirists, or travelers, within a single chapter. Such a method requires the author to connect the sketches carefully by means of a narrative or an argument, and this Alpatov fails to do.

The treatment of intellectuals outside of Russia is largely unsatisfactory. Alpatov has an inflated sense of the ascendancy of French thought in the eighteenth century; English and German thinkers are dismissed in a few summary pages. Yet even his characterization of the French Enlightenment is a largely formulaic and cliché-ridden disquisition on "the age of reason."

At times Alpatov's Continental perspective deserts him altogether, and in its place he advances

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extravagant claims for the worth of Russian thinkers. M. V. Lomonosov's championing of anti-Normanism, for example, is praised as a major advance in the scholarship of early Russian history. Fair enough. But then Alpatov takes us on a journey through Lomonosov's views on ancient Greece and Rome, which he concludes with the astonishing observation that "subsequent bourgeois historiography of the nineteenth and especially the twentieth century added nothing fundamentally new to [Lomonosov's] picture of the decline of the Roman empire. And as a rule it went backward" (p. 69). Equally overdrawn is his discussion of the eighteenth-century Siberian journal *Irtysh*, which he suggests manifested strong sympathies for the French revolution.

A final problem concerns the author's failure to engage seriously the voluminous existing scholarship, Soviet or Western, that relates to his subject. The historiographic studies of S. L. Peshtich, for example, are not mentioned, and the monumental works of P. N. Berkov, Iu. M. Lotman, M. M. Shtrange, among others, are discussed only in passing.

This study, then, can be commended for a few of its contributions. Overall, however, it does not make a substantial contribution to Russian intellectual history.

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HANS ROGGER. *Jewish Policies and Right-Wing Politics in Imperial Russia*. Berkeley and Los Angeles: University of California Press. 1986. Pp. viii, 289. $25.00.

For the past twenty years Hans Rogger has been exploring anti-Semitism and right-wing politics in imperial Russia. By bringing together in this book six of his articles and a new introductory chapter, he demonstrates how thoughtfully and consistently he has considered his subject during this time. Although he has incorporated the most recent scholarship into his essays, he finds that it forces no change in his essential argument.

The theme that unites the essays is the disturbing question of why the virulent, Russian form of anti-Semitism resisted emancipation of the Jews so much more stubbornly than did Western anti-Semitism. He assures us that he has no easy answers. Fear and prejudice combined with objective factors. A relatively large ethnic group cramped into the small area of the Pale, the Jews were trapped in an underdeveloped economy with a peculiar social structure. Every action they took to better their lot rebounded against them. If they acquired land they were accused of taking bread from the peasants; if they went into commerce they were sucking their blood. If the Jews sought to assimilate they remained objects of discrimination; if they clung to their traditions they were denounced as alien. Anti-Semitism in Russia came close to racism, Rogger tells us, without ever completely embracing the idea. He dismisses the notion, popularized by Norman Cohen, that anti-Semitism was the outcome of a drive for national or religious unity. Yet he does not deny that profoundly irrational elements in the Beilis case, for example, drove government officials to experiment with anti-Semitism—as "a passion and conception of the world" in the words of Jean Paul Sartre. He agrees with Peter Struve that there was throughout Russian society "a strong feeling of repulsion for the Jews." The bureaucrats were suspended between their fear that the Jews would economically enslave the helpless peasant and their fear that pogroms unleashed by the peasants would mean chaos for Russia, "the black revolution." Their response was to avoid any extremes in legislation; they sought neither to assimilate the Jews nor to expel them but, rather, to muddle along. They passed a mass of legislation that followed "a recurrent pattern of harshness alternating with minor concessions" (p. 112). Even the moderates, who were mainly ministers of finance, sought to alleviate the worst restrictions on the Jews only for tactical reasons. The rise of anti-Semitic parties after 1905 fell short of breeding true fascists, Rogger insists. Russia had not yet been radicalized by a world war, its middle class was too weak to provide a respectable mass base for the fanatics in the Union of the Russian People, and the tsar on the throne made nonsense of *Führerprinzip*. As his introduction suggests, Rogger perceives the problem of Jewish emancipation as part of the general malaise of the Russian empire. The state was neither multinational nor unitary; behind the drive for industrialization lurked the shadows of anticapitalism; the rights of citizens clashed with the concerns of a paternalistic bureaucracy. The Jews suffered from every one of these contradictions.

Rogger admits that his study lacks an analysis of the popular sources of anti-Semitism that underlay high politics. One wishes he had tried his hand at it, for no scholar in the field commands as broad and balanced view of this controversial problem as he does.

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SEYMOUR BECKER. *Nobility and Privilege in Late Imperial Russia*. Dekalb: Northern Illinois University Press. 1985. Pp. xiv, 259.

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In this well-researched book, based on unpublished archival records of the Special Conference on the Affairs of the Noble Estate (1897–1902) and on a wide variety of statistical data, Seymour Becker studies several crucial aspects of the history of the Russian nobility from 1861 to 1914: the changing relationship of the nobility to the land, the campaign waged by "self-appointed traditionalist defenders of noble interests," and "the emergence of a *class* of substantial landed proprietors out of the noble *estate*" (p. xiii).

At the beginning of his book Becker challenges the conventional view of the nobility as a social estate in the process of rapid decline. The nobility, he writes, "was not the passive victim of its own pathology and of external forces beyond its control" but was "an active participant in the process of adjustment to changing conditions" (p. 13). Becker admits that the majority of nobles divested themselves of their estates after 1861 but says they often did so because they could invest their capital more profitably in nonagricultural enterprises. Few nobles lost estates by failing to meet mortgage payments; in fact, they viewed mortgages from state lending institutions as easy sources of capital. Becker sees the minority of nobles who kept their land as increasingly "committed agrarian entrepreneurs" (pp. 53–54). His description of how nobles responded to market conditions is clear and convincing in most respects. His notion of landowning nobles as agrarian entrepreneurs, however, needs qualification. As he admits, at the close of the nineteenth century three-quarters of the nobility's arable and meadows was leased to peasants. One does not generally classify rentiers as committed entrepreneurs.

Chapters 3 to 7 treat traditional noble ideology and the campaign by traditionalists to halt the "decline of the nobility." These chapters sit uneasily with the first section of the book. After all, if no objective crisis affected the nobility, then why did the traditionalists lament the dangers threatening the first estate? Becker's answer to this difficulty is that the traditionalists represented only a tiny fraction of nobles "who found Russia's evolution away from hereditary privilege and toward legal equality both distasteful and alarming" (p. 57). The traditionalists tried to preserve the landed nobility by insulating it from market forces, restricting access to noble status, guaranteeing preferential treatment of nobles in government service, separating nobles from nonnobles in schools, and maintaining the local authority of nobles through institutions such as the land captains and noble marshals. In nearly every case the traditionalists failed to persuade the government to adopt their program. When they succeeded in winning changes in the law (as in the law of 1899 on entail), the vast majority of landowners ignored the changes. "By their lack of enthusiasm for schemes designed to protect them against social change," Becker notes, "the overwhelming majority of the remaining noble landowners demonstrated that they were not interested in becoming a fossilized social element, insulated from the free play of market forces" (p. 173). The issues discussed in these chapters have been treated at length by other historians of the nobility; Becker's contribution is to reinterpret the familiar material in light of his conviction that the nobility was not in decline.

In the third and weakest section of the book Becker examines noble politics from 1905 to 1914 and claims that in this period "estate consciousness" gave way to growing "class consciousness" on the part of large and intermediate landowners. "Interest-group politics based on class drew more response from substantial landowners than the schemes of the [traditionalist] *soslovniki* . . . ever had" (p. 173). Becker's conclusion rests on his understanding of the United Nobility as an economic interest group representing the 12 percent of the nobility whose families owned substantial quantities of land. In fact, the United Nobility was a more marginal and unrepresentative body than Becker imagines; furthermore, to describe its ideology as fundamentally different than that of the traditionalists of the 1890s is unjustified. Becker's concluding observations about the autocracy's culpability (and the nobility's relative lack of culpability) for the Russian revolution of 1917 are strange and unenlightening. Even less enlightening is the sudden reference to modernization theory as an explanation of Russia's historical course (pp. 174–75).

In the end, Becker's book is most valuable as a social history, establishing a quantitative portrait of the late nineteenth-century Russian nobility. As political analysis, the book is curious but unconvincing.

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BRIGITTE LÖHR. *Die "Zukunft Russlands": Perspektiven russischer Wirtschaftsentwicklung und deutsch-russische Wirtschaftsbeziehungen vor dem Ersten Weltkrieg.* (Historische Forschungen, number 11.) Stuttgart: Franz Steiner. 1985. Pp. 243. DM 84.

Brigitte Löhr's book is about German perceptions of Russia's economic condition during the decade before World War I. Relying chiefly on the writings of diplomats, state officials, financiers, and businessmen, she traces a broad development in the arguments of German experts who counseled against investment in Russia. Before 1909 they argued from Russian backwardness and inherent economic weakness; after 1909 they spoke of Russia's striking success at economic modernization but saw its growing strength as a threat to Germany and a pitfall for

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German investors. In early 1914, when Russia appeared to be facing an economic crisis, most doom cryers did not return to the earlier image of backwardness but instead focused on the apparent subservience of Russia to French influence. Russian power in crisis appeared even more dangerous to Germany than in boom times.

Löhr seems reluctant to extract sweeping conclusions from the tendencies she traces. On the contrary, by stressing the extreme diversity of interests that made up what are commonly called "reactionary" and "liberal" tendencies in Russian government, she creates considerable discord in these old harmonies. Following the lead of Walther Kirchner's seminal article (*Journal of Economic History* 1981 [41]: 361), she indicates that foreign investors held widely differing opinions on the Russian government. Different branches of industry supported different economic philosophies; individual firms within each branch did not always take the same view as the organs representing the industry as a whole. In 1908, for example, German chemical firms with branch plants in Russia actively supported "reactionary" tariffs, whereas German exporters of chemical products strove mightily in behalf of the "liberal" antitariff cause (pp. 81–82). In late 1905, following the publication of the October Manifesto, "revolutionary" workers and peasants in the south and southwest slaughtered Jews and burned out their businesses on an unprecedented scale, bringing virtually all trade to a halt. Russian credit fell to its lowest ebb (pp. 64–66). Somehow, however, the distribution network recovered. German investors, among others, continued to sell on credit in spite of all the solid reasons for staying clear of the maelstrom. In short, German investors in tsarist Russia cannot be usefully characterized by a consistent political ideology or measures in accordance with abstract concepts of economic interest.

It is refreshing to read a book on economic history in which the author does not labor to fit data into preconceptions. Löhr's study fails to explain German investment in tsarist Russia, but perhaps this is just as well. Perhaps capital investment is a much more mysterious phenomenon than we have realized. If Löhr's account does not make clear whether the tsarist government dominated Russia's creditors or vice versa, it is because the issue is far from decided. If Löhr cannot explain why German investors put so much capital into tsarist Russia in disregard of an abundance of expert, well-reasoned warnings, it is probably because no satisfactory explanation is as yet available. Löhr is to be praised, not blamed, for not offering generalizations.

GEORGE YANEY
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D. A. KOLESNICHENKO. *Trudoviki v period pervoi rossiiskoi revoliutsii* [The *Trudoviki* in the First Russian Revolution]. Moscow: Nauka. 1985. Pp. 317. 3 r.

In the past decade Soviet historians have paid greater attention to the "populist" parties, which were among the Bolsheviks' main rivals in the early twentieth century. Some of the best of these studies (notably V. N. Ginev's work on the Socialist-Revolutionaries [SRs] and N. D. Erofeev's on the Popular Socialists) have shown a degree of understanding of and sympathy for their subjects that was unthinkable for earlier Soviet historiography. D. A. Kolesnichenko's writings on the *Trudoviki* also fall under this category. The author of a doctoral dissertation and a number of articles on the *Trudoviki* in the First Duma of 1906, she has now extended her work into 1907 and the period of the Second Duma.

The State Duma, the quasi-constitutional legislative assembly created as a result of the tsar's October Manifesto of 1905, was elected on the basis of a complex indirect voting system, involving a property franchise, which nonetheless gave the peasantry the right to elect about 45 percent of the deputies. The government had apparently calculated—wrongly, as it turned out—that the peasants would vote for conservatives. In the First Duma a group of about one hundred deputies, the majority of them elected by the peasant constituencies, constituted the Labor Group (*Trudovaia gruppa* or *Trudoviki*). In the absence of any significant representation of the revolutionary and socialist parties, which had mostly boycotted the elections, the *Trudoviki* were the most substantial leftist faction in the Duma, and they quickly became the objects of competing political influence from parties both inside and outside the Duma. After the dissolution of the First Duma, the *Trudoviki* fought the elections to the Second Duma as part of a "left bloc," and in the Duma itself they formed a united fraction with the representatives of the All-Russian Peasant Union. The socialist parties, which had dropped their boycott, were represented in the Second Duma, but attempts to get the *Trudoviki*, the Popular Socialists, and the SRs to act together as a single populist bloc, let alone to form a left bloc including the Social Democrats, ended in failure. The land question was of particular interest to the peasant deputies. In the First Duma more of the *Trudoviki* supported the agrarian proposals prepared by the Popular Socialists (the Draft of the 104) than those of the SRs (the Draft of the 33). The SRs, however, succeeded in obtaining greater support from the *Trudoviki* for their proposal for land reform in the Second Duma than they had in the First.

Kolesnichenko's study of the *Trudoviki* shows their social composition and demands to have been more truly representative of the peasantry than those of

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either of the other populist fractions. The author sees them as the embryo of a genuinely peasant party—a status, however, that they failed to obtain in 1906–07, when they remained a group of Duma deputies, with no real organization in the country as a whole. The *Trudoviki* saw themselves as a nonparty and supraclass organization, representing the interests of "labor" in the populist sense of a broad alliance of peasants, workers, and intelligentsia. Kolesnichenko's interpretation of their significance, however, is naturally heavily influenced by Lenin's writings. The Bolshevik leader regarded the *Trudoviki* as the representatives of the "revolutionary-democratic" elements among the peasantry with whom the proletariat could form an alliance, and Kolesnichenko judges their actions from this Leninist persective. Within these limitations, however, the study is interesting and informative and is based on a wealth of archival material that has not previously been used by historians.

MAUREEN PERRIE
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S. A. KRASIL'NIKOV and V. L. SOSKIN. *Intelligentsiia Sibiri v period bor'by za pobedu i utverzhdenie Sovetskoi vlasti (1917–leto 1918).* [The Intelligentsia of Siberia during the Period of Victory and Consolidation of Soviet Power (1917 to Summer 1918)]. Novosibirsk: Nauka. 1985. Pp. 253. 2 r. 60 k.

This rewarding book, printed in only 1,350 copies and intended for historians, is concerned with the Bolsheviks and Siberian intellectuals from the fall of the monarchy to the summer of 1918, when the Whites took control of Siberia. Relying on published and archival sources as well as materials in private collections, S. A. Krasil'nikov and V. L. Soskin describe the reactions of various Siberian educated groups to the events of 1917 and 1918. The story is told in three phases: from February to October 1917, the October revolution, and the period from October 1917 until the summer of 1918.

Between the February and October revolutions most Siberian intellectuals opposed the Bolsheviks. The Siberians had "illusions of constitutionalism," meaning they believed the Constituent Assembly could avoid class warfare, and they supported the war. Only a few supported the Bolsheviks, but many, considering themselves above politics, professed neutrality. In this period the Bolsheviks sought to stimulate neutralism among the majority while attracting the "half-proletarian performers of mental labor" to their cause.

This same pattern persisted after Soviet government was established in October. But the Soviets could now freely use force against the "bourgeois bosses" who openly opposed Soviet rule. Further, the Soviets now desperately needed educated specialists to help them govern. Maintaining the flow of food from Siberia to European Russia was also essential. In this period the neutrality of the bulk of the intellectuals combined with fear of unemployment to induce them to work for or with the Soviet authorities. Between 1917 and the following summer Soviet authority was established, only to be overturned by the Czechoslovak revolt.

Krasil'nikov and Soskin furnish much information on the Siberian periodical press in 1917 and 1918, on Siberian autonomism (*oblastnichestvo*), and on the attitudes of groups in Siberian cities. The book is not, however, faultless. The sociology is distorted to fit Soviet preconceptions, so that Mensheviks and Socialist Revolutionaries become bourgeois, and professors, lawyers, and engineers become "bourgeois bosses." Refusal to recognize Soviet power is labeled "sabotage."

But there are encouraging signs. A book by the anti-Communist G. K. Gins, a member of A. V. Kolchak's government, is openly cited. In general, books are listed by author and title rather than as items in archival *fonds*. Applying a technique rare in Soviet scholarship, the authors use content analysis to ascertain the political orientation of Siberian periodicals. This book is a valuable contribution to historical understanding.

PATRICK R. TAYLOR
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ALFRED EISFELD. *Deutsche Kolonien an der Wolga 1917–1919 und das Deutsche Reich.* (Veröffentlichungen des Osteuropa-Instituts München, Geschichte, number 53.) Wiesbaden: Otto Harrassowitz. 1985. Pp. 177. DM 44.

Alfred Eisfeld's study focuses on the Volga Germans during two tumultuous years between the outbreak of revolution in Russia in February 1917 and the creation of an autonomous Volga German district in April 1919. Eisfeld's effort is welcome because this theme touches on complex issues during a crucial phase of the revolution and has received little scholarly attention.

Eisfeld proposes to determine who among the Volga Germans were bearers of the revolution, the nature of their motives, the impact of economic conditions on colonist attitudes, and the role the colonists played in the objectives of the German empire. Yet his is a standard political history of the Volga Germans during the revolution and Civil War. Eisfeld concentrates on colonists' conferences, missions to Berlin, and discussions with Soviet authorities. In Eisfeld's judgment, despite tsarist at-

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tempts at russification, Volga Germans turned to Germany only after revolutionary confiscation of grain, livestock, and property. The collapse of the German empire dashed hopes for resettlement or occupation of Volga German districts by the German army. Ultimately, Volga Germans had no choice but to negotiate autonomy on the Bolshevik regime's terms.

Eisfeld often subordinates discussion of Volga German concerns to an analysis of activities and aspirations of German colonists elsewhere in Russia. Such broader considerations were probably unavoidable; however, Eisfeld covers neither the narrower Volga German topic nor its wider setting adequately. Eisfeld says that his more inclusive approach resulted from failure to gain access to relevant Soviet and East German archives. This also explains why the author concentrates on political analysis at the expense of the important social issues raised in his introduction.

The text is appended with a series of well-selected documents. A map would have been useful.

BENJAMIN L. BENFORD
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PHILIP POMPER, editor. *Trotsky's Notebooks, 1933–1935: Writings on Lenin, Dialectics, and Evolutionism.* Assisted by YURI FELSHTINSKY. New York: Columbia University Press. 1986. Pp. viii, 175. $25.00.

The two notebooks and clutch of additional notes that constitute the foundation of this book are modest in bulk, roughly seven thousand Russian words. These materials from the Trotsky papers at Harvard University are meticulously presented in both the original Russian and English translation, with an introductory description of their physical format, footnotes in Russian on peculiarities of the text, such as grammatical errors and color underscoring, and explanatory footnotes in English on Trotsky's allusions, mainly to people. Scrawled in pencil, with many abbreviations, the notebooks presented problems of transcription, which Yuri Felshtinsky has solved. Of about thirty-eight pages of the notebooks in translation, about eight concern Lenin and history—preliminary jottings for Trotsky's intended but unwritten life of Lenin. The rest consists of a scattering of thoughts on dialectical philosophy and science, which in Trotsky's mind formed a single category. Philip Pomper shows that this was inspired largely by Max Eastman's challenge to Trotsky's Hegelian convictions.

Although this archival material by itself would interest only a few specialists, Pomper's introduction, which occupies almost half the volume, endows the notebooks with high intellectual appeal. A master of the primary and secondary literature on Trotsky, he places the notebooks in their biographical context and offers an analysis of the revolutionary's "dialectical style" in contrast to that of some of Trotsky's Russian Marxist contemporaries. Among Pomper's observations concerning matters biographical, I found particularly apt the suggestion that Trotsky was attempting to characterize himself in his descriptions of Lenin's strengths. On dialectical style Pomper sees Trotsky as emphasizing the "catastrophic principle," in contrast to Lenin's espousal of "flexibility."

Without losing his critical balance, Pomper is highly respectful of Trotsky's intellect, which has justifiably impressed many observers, Trotsky among them. Concerning Trotsky's vanity and his notebooks on dialectics and evolutionism, there is one basic question (perhaps too elementary to Trotsky and Pomper to prompt either of them to address it at length) that I wish Pomper's essay had explained for me. Did Trotsky see the dialectic as a heuristic device only? Or did he believe that, with his self-taught knowledge of science, based partly on newspapers, he could discover how the dialectic works as a kind of "unified field theory," embracing all the natural and social sciences?

ROBERT H. MCNEAL
University of Massachusetts

NEAR EAST

RALPH S. HATTOX. *Coffee and Coffeehouses: The Origins of a Social Beverage in the Medieval Near East.* (Near Eastern Studies, number 3.) Seattle: Department of Near Eastern Languages and Civilizations, University of Washington; distributed by University of Washington Press, Seattle. 1985. Pp. xii, 178. $9.95.

The discovery in Ethiopia or the Yemen that the coffee bean could be used for brewing a pleasant and harmless hot drink was a noteworthy service for human society. According to documented evidence, it happened in the fifteenth century. From Arabic sources and reports by European travelers in the Near East, Ralph S. Hattox has put together the available information and discussed its significance in great detail. In particular, he wishes to point out an important transformation—if transformation is not too strong a word—in Near Eastern society as the result of the introduction of coffee. In the Muslim environment coffee had three great virtues. It was a stimulant that caused no real physical effects (and those it did were mostly in the imagination), in contrast to wine, which was outlawed by the Koran. It was conducive to socializing, in contrast to other drugs that were, or were considered, introverting and normally tended to make their users shun

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preachers with council. Finally, a conclusion would have given the book a more finished appearance.

These criticisms should not detract from McLaughlin's achievement. He has produced the richest and best historical biography of Schwenckfeld to 1540. In constant debate with previous scholarship, he advances our understanding of Schwenckfeld's intellectual evolution and enriches our appreciation of the complexity of the Reformation as a whole. Apart from a few printing errors (for example, the omission of note 50 on page 280), the volume makes excellent reading. It is a very welcome contribution to our understanding of this reluctant radical, whose descendents sought refuge on North American shores.

WERNER O. PACKULL
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RICHARD VAN DÜLMEN. *Die Gesellschaft der Aufklärer: Zur bürgerlichen Emanzipation und aufklärerischen Kultur in Deutschland*. Frankfurt: Fischer Taschenbuch. 1986. Pp. 204. DM 12.80.

This slim but important volume surveys the establishment and development of various "enlightened" societies, associations, and clubs in eighteenth-century Germany and analyzes their differing (and evolving) social composition, goals, and relationship to public institutions. These societies, taken together, were perhaps the most important means by which the German middle classes (*Mittelstand*) gained the group self-consciousness and self-confidence that underlay their gradual liberation from the sociopolitical constraints of the traditional "feudal" order. This liberation, Richard van Dülmen asserts, was accomplished by means of education (*Bildung*), but education in the sense of a purely individual self-cultivation cannot become a social force. For that to happen, it must be aggregated. The organizations dealt with in this book, as new forms of communication between heretofore isolated and separated elements of German society, including not only the various middle-class occupational groups but also nobles, officials, clerics, and so on, were precisely the means by which such aggregation occurred. Once in being, however, aggregation not only stimulated the elaboration of the educational process but also, in socializing that process, led to a gradual reformulation of the nature and goals of education itself. From this, in the course of the century, emerged a bourgeois elite that was self-confident not only culturally but also socially and politically.

The author distinguishes three distinct phases in the evolution of "enlightened" societies. Each phase (corresponding roughly to categories of early, mid-, and late Enlightenment) was earmarked by the emergence of new societies—which did not, however, replace earlier ones—with different social compositions, socializing rituals, agendas, and attitudes toward state authority. To oversimplify somewhat, each new phase was characterized by a widening social composition (with a pronounced tendency toward increased bourgeoisification), more "democratized" social interaction within the societies, an increasing orientation of agendas toward reform, including political reform, of institutions and practices in the real world, and a tendency toward greater independence from, and even hostility to, existing political authority. From the formal learned academies and purely literary societies of the early eighteenth century, through the "patriotic societies" and Freemasonry of midcentury, to the Illuminati and radical *Volksgesellschaften* of the 1790s—all of which are catalogued in an appendix—the author provides a solid, fascinating, and comprehensive survey of an important aspect of the "emancipation" of the German bourgeoisie, which has recently been the subject of much discussion and research.

JOHN G. GAGLIARDO
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ARLIE J. HOOVER. *The Gospel of Nationalism: German Patriotic Preaching from Napoleon to Versailles*. Stuttgart: Franz Steiner. 1986. Pp. 164. DM 38.

Since 1945 German Protestants have harshly judged traditional German nationalism. But no comprehensive history of religious nationalism in the Protestant *Landeskirchen* has yet appeared. Arlie J. Hoover's excellent book may be regarded as a contribution to such a study. He has dissected thousands of Protestant sermons preached during the War of Liberation, the Franco-Prussian War, and World War I.

According to Hoover, the rationalism of the Enlightenment, pietism's antipathy to dogmatism, and the epistemology of Kant converged by the time of the War of Liberation to undermine the dogmatic content of Lutheranism. Reacting to the humiliation of Napoleon's domination, pastors filled this void in Protestantism by conceiving of a romantic concept of the German folk. Following Koppel Pinson, Hoover argues that pastors endowed the folk with pietism's emotionalism and religious conceptions. Thus, they could conceive of the folk as a divine creation led on a special mission by divine providence.

Hoover may well have overstated a good case. Pietism was not entirely devoid of dogmatic content, and pietism allied with an aroused Lutheranism to dominate much of church life (but not academic theology) for much of the century. The question is

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whether he need hypothesize a dogmatic void to hypothesize a transferral of religious categories to a conception of nationalism. Moreover, pietistic conservative Lutherans suspected folkish religious nationalism for its political implications even after the era of German unification, and one must ask whether religious nationalism actually took the clergy by storm.

Much of the book analyzes the themes of religious nationalism. Pastors claimed that God favored the German folk in these wars. The author illustrates how pastors judged the cause of the folk in religious categories, such as by sensing in patriotism the infusion of the Holy Spirit. But why did even clergymen who subscribed to Reformation theology fail to sense a contaminating, competing religion in religious nationalism?

By Hoover's account, the content of religious nationalism had grown more secular by World War I. But Walter Bredendiek (in *Irrwege und Warnlichter*, [1966]) saw a far greater change by then, proposing that many pastors regarded war as a positive good. To what extent did religious nationalism change qualitatively from war to war? Moreover, wartime may have called forth an extreme religious nationalism, whose acceptance only grew in stages throughout the century. Whether a focus on wartime sermons obscures a more general development of religious nationalism is a further question.

Hoover's trenchant analysis of sermons is similar to results published by German scholars. But only his book compares these sermons in three wars over a century and, therefore, can show a high degree of continuity. Bredendiek's book as well as the following recent collections of valuable essays should be added to the bibliography: *Volk—Nation—Vaterland*, edited by Horst Zillessen (1970) and *Kirche zwischen Krieg und Frieden*, edited by Wolfgang Huber and Johannes Schwerdtfeger (1976).

DANIEL R. BORG
Clark University

HEINZ HÜRTEN. *Kurze Geschichte des deutschen Katholizismus 1800–1960*. Mainz: Matthias Grünewald. 1986. Pp. 280.

The title of this book may be a little misleading, especially to American readers, since its subject is not the Catholic church as such but rather the structure and activity of Catholic associations, what is known in Germany as *Vereins-* or *Verbandskatholizismus*. More an interpretive essay than a comprehensive history, the work is organized around two main themes: the associations' influence on society and politics, especially on the conflict between church and state, and the relationship between lay and clerical elements in the groups' functions, leadership, and internal organization.

Heinz Hürten begins his account in the early nineteenth century, when the destruction of the Old Regime in the wake of the French revolution had disrupted the organization of the Catholic church in Germany and left it unable to defend its institutional autonomy against hostile secular states. With a nonexistent or politically paralyzed ecclesiastical leadership, a small group of lay activists had to find ways of mobilizing mass support in defense of the church's interests. Hürten discusses the early forms of Catholic organizations, their intellectual justification, and their further development: the successful integration of clergy and episcopate into their activities, their response to political events and to the emergence of the social question in Germany, all culminating in the great, successful struggle against state intervention during the Kulturkampf.

Most of this will be familiar to readers of Karl Buchheim's *Ultramontanismus und Demokratie* (1963), but, in the chapters on the period after 1890, Hürten emphasizes his other main theme, explaining how Catholic organizations successfully warded off efforts at tightening episcopal control before 1914. After World War I, however, the author sees a growing tendency toward clerical leadership and a more church-centered organization embodied intellectually in the liturgical movement of the 1920s and organizationally in the Vatican-sponsored Catholic Action. This theme is also important in his discussion of Catholic organizations during the Nazi era, besides the more obvious topic of state persecution. Hürten's brief final section on the post-1945 period is centered around a discussion of the gradual dissolution of the world of autonomous Catholic associations and their incorporation into church organization (*Verkirchlichung*), which the author believes has substantially weakened Catholic influence on state and society in the Federal Republic.

Even given the work's character as an interpretive essay, I have to wonder if the author's main themes are adequate for his purposes. Focusing exclusively on conflict between church and state ignores the many instances of cooperation, which are important for understanding the nineteenth-century Catholic movement. The virtual lack of any detailed consideration of social and economic factors hinders understanding of the activities of Catholic trade unions and peasant associations or clerical attitudes toward them; long-term tendencies toward secularization in twentieth-century German society or the growing importance of the mass media in influencing public life may have had as much to do with the declining influence of Catholic associations as have organizational changes. In sum, I found the author's interpretation concentrated too much on the organizations themselves and too little on the social and

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political world they sought, often successfully, to influence.

JONATHAN SPERBER
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DAN SIMON. *Das Frankreichbild der deutschen Arbeiterbewegung 1859–1865*. (Schriftenreihe des Instituts für deutsche Geschichte Universität Tel Aviv, number 6.) Gerlingen: Bleicher. 1984. Pp. 334.

The economist Max Wirth, engaged with his brother Franz in creating a national businessmen's newspaper, was also stirred by international events of the years 1859 and after. He wrote of the "criminal conduct" of the French in opposing German unity. He found that nation "decadent" and "illiterate," its finest sons having perished as Germans in the St. Bartholomew's Day Massacre. "The basis of his judgment of events was his solid belief in the superiority of the German race over the Romanic," relates Dan Simon (p. 38). Wirth believed that a brilliant future lay before humanity as soon as it came under German sway but that the French were likely to languish in the grip of hundreds of thousands of Fourierists.

Less frantic than Wirth, but still recognizing France as the enemy, was Leopold Sonnemann, then involved in founding the *Frankfurter Zeitung*. His great phobia was a French attack on the Rhine. Less well known, but more fanatical than any other of Simon's chosen was Nikolaus Hadermann, who denounced the French in his own small paper as a "gang of robbers," dreaming day and night of a Rhenish boundary. He maintained that the Gallic neighbors, a people without culture, had only sent troops to Italy to damage Germany.

In Simon's view Ferdinand Lassalle was more rational than the others. He respected French culture, supported Italian independence despite its French support, and did not expect that the armies of Louis Napoleon would reach the Rhine before daybreak. But Simon does not concentrate on Lassalle, and he moves on to white raven Moses Hess. Hess had working-class credentials, but he was a Francophile, even an admirer of Louis Napoleon. He made a flier to Cologne to help Lassalle, then quickly returned to more congenial Paris. Had he swallowed too much hostility toward the land he felt should lead the world?

Simon's evidence of German hatred for the Gallic foe, prevalent and virulent already in the 1860s, is the most impressive side of his book. That hatred, he demonstrates, was shared by men of intelligence and position. But Simon beclouds the matter by insisting that these attitudes have something to do with the working class. Karl Marx, Friedrich Engels, August Bebel, and Wilhelm Liebknecht devoted decades of their lives to working-class politics, but their opinions of things French are not found here. Even the unreliable J. B. Schweitzer spent more years in proletarian meetings than did Lassalle but is not quoted. Why are these men neglected when the nonentity Hadermann is given thirty pages? Even worse, there is nothing from workers themselves. Simon asserts, near the beginning, that their ideas on France would be difficult to uncover. No doubt they would, especially given the predilection of laboring people to leave foreign affairs to their betters. But the task would not have been totally impossible, and the results might well have been illuminating.

RICHARD W. REICHARD
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NICOLAS TEEUWISSE. *Vom Salon zur Secession: Berliner Kunstleben zwischen Tradition und Aufbruch zur Moderne 1871–1900*. (Jahresgabe des Deutschen Vereins für Kunstwissenschaft.) Berlin: Deutscher Verein für Kunstwissenschaft. 1986. Pp. 333.

For approximately the past two decades, art historians both in the United States and abroad have attempted to "read" the texts of art and architecture—paintings, graphics, sculpture, and buildings—and to put these "texts" in historical, political, cultural, and social context. This effort can be seen with greater frequency. For example, major museum catalogues and monographs on individual artists or aesthetic movements invariably contain historical material as part of their volumes. The *Journal of Interdisciplinary History* and the *Art Journal* have recently devoted issues to the theme of the intersection of art and history. Nicolas Teeuwisse's study belongs to the growing genre of "art in context" monographs. As such, it provides an amply documented and intelligent summary of imperial Berlin's artistic culture around the turn of the century.

Teeuwisse organizes his work chronologically. He outlines the main features of Berlin's art scene decade by decade, beginning in the 1870s after German unification and ending with the turn of the century. The major focus of his discussion is the growing antagonism between conservative academic painters of official Berlin, such as Anton von Werner, and the "modernists," such as Max Liebermann, whose works received increasing publicity and attention from museum officials and individual collectors. Werner was not only one of the most important "court painters" associated with Emperor William II but also head of the Royal School of Fine Arts and the influential Artists' Association (Künstlergenossenschaft). Liebermann, in addition

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to being Germany's leading impressionist painter, helped found and was elected the first president of the Berlin Secession, an elite artists' organization devoted to the display of German and international modern art. These two painters, representing conservatism and modernism, served as foils for important aesthetic, cultural, and even political questions in turn-of-the-century imperial Germany.

A number of scholars have previously presented the dramatic confrontation between the old and new (notably in the series edited by Ekkehard Mai and Stephan Waetzold, *Kunst, Kultur und Politik im deutschen Kaiserreich*, Teeuwisse in his essay for a museum catalogue on Liebermann, 1979–80, Werner Doede in his lavishly illustrated *Die Berliner Secession* [1977], and Peter Paret's monograph on the same subject). Teeuwisse's monograph is more ecumenical in its subject matter—detailing the activities of Berlin's academic painters, art dealerships, museums, and art associations—and discusses the battles fought between conservatives and progressives over the direction German art should take. But, given the complex and changing structure of Berlin's late nineteenth-century art world, Teeuwisse was forced to sacrifice depth for schematic coverage. For example, the author mentions, yet hardly indicates the importance of, the pivotal retrospective exhibition of nineteenth-century German art at the Nationalgalerie in 1906 and the role it played in subsequent interpretations of German painting. Aside from this minor reservation, Teeuwisse succeeds in highlighting the crucial episodes in Berlin's transformation from a "barracks town" to a true artistic metropolis. Teeuwisse is to be congratulated for using a wealth of archival and secondary material to good advantage. This material illuminates the aesthetic contributions of the major German artists residing in turn-of-the-century Berlin and situates their works in their cultural and historical context.

MARION F. DESHMUKH
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HELMUT AHRENS. *Das Leben des Romanautors, Dichters und Journalisten Theodor Fontane*. Düsseldorf: Droste. 1985. Pp. 439.

The title of this book, had its author preferred a more archaic mode, might well have been "The Life of the Apothecary, Poet, Novelist, Historian, Press Attaché, Journalist, Academy Secretary, Theater Critic, etc. Theodore Fontane." Helmut Ahrens, who has previously published popular biographies of Johann Nestroy and Ludwig Thoma, certainly covers all of these aspects of Fontane's life well and entertainingly. His book, as he generously acknowledges, owes much to the recent work of Helmuth Nürnberger and Walter Seidel, but even more to Fontane's own writings, and Ahrens is wise enough to let Fontane himself do much of the talking. Thus, we hear Fontane's own words on the money worries that constantly plagued him. His two illegitimate children, he wrote early, "are eating the hair off my head before the world even knows that I have any" (p. 112), and, despite his growing reputation, he found no true financial security until he was well into his fifties. Throughout, Fontane kept his good humor and his good sense. They show up, for instance, in his observations about history that Ahrens quotes and that seem as fresh and relevant as ever.

His main interest was always in *petit histoire*. Large events, he wrote, would never lack for chroniclers; he was after the "middling, even small occurrences," which he found both more intriguing and more revealing. His *Wanderungen durch die Mark* overflowed with them, of course, and his love of detail and of a good anecdote still makes this book as much of a joy to read as his novels. Besides, he was a skillful practitioner of oral history before that term had even been invented. Ahrens cites one memorable episode in which Fontane asked his sister to find out what she could about the murder of a Frenchman in the village of Dreetz in Ruppin County, which, according to the documents, had occurred in 1806. "It does not have to be true," he wrote her, "*vox populi* has the right to lie as much as it wants. It is called a 'myth' in that case and is treated with the highest respect by scholars and oddballs like me. So please drum up a few people in Dreetz: perhaps some shepherd's helper of descending mental development, some 'wise woman,' some soothsaying imbecile. Blast it all, one of them should have enough imagination to discover why the poor Frenchman got himself killed. I myself can and may not invent anything, for one thing because it is against my 'historical conscience,' and for another because I am really convinced that some upstanding citizen of Dreetz in truth killed the Frenchman the way one drives a stick into the ground, or for even less reason" (p. 200).

Fontane's little history, of course—in his journalism, his travel books, his novels—has given us truths that have escaped writers with far greater academic credentials than his apothecary's diploma. This has been particularly true, as Ahrens once again reminds us, of his observations on Prussia. As much as anyone else (including Heinrich von Kleist, whose Prussia really was a dream Prussia) he was the chronicler of its positive sides, beginning with his ballads celebrating Prussia's heroes, and ending with the figure of Dubslav von Stechlin. "He had the . . . self-confidence of those who had been there before the Hohenzollern, but it was a very quiet self-confidence, and if he ever did express it it was in the

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form of humor and self-irony, since it was his nature to provide everything with a question mark. His finest feature was a heart-felt humanity, and about the only things capable of upsetting him (who otherwise would humor anyone who thought that five was an even number) were pride and arrogance" (p. 391). Yet Fontane could also be vocal indeed about Prussian weaknesses, whether it was the signs of arrogance that Stechlin so detested or its shortage of imagination or generosity. "Everyone walks around with a blue cornflower in their buttonhole here, to show respect for the king by way of his favorite flower," he wrote to his wife from Berlin on the king's birthday in 1879. He did not find the gesture endearing. "It is a leathery flower," he wrote, "being blue is its sole virtue. It has no fragrance, no beauty, no poetry. It is as though it had been designed specially for us; all that's missing on it is a red trouser stripe" (p. 291).

Yet an air of tolerance, Stechlin's permanent question mark, pervaded all these comments, no matter what side Fontane's shifting moods put him on. As a young man, Fontane had been a tutor (one of the several occupations covered by the "etc." of the suggested alternate title) in the Wangenheim household. There, he was particularly impressed by something Dubslav von Stechlin would have loved. Baron von Wangenheim, a senior official in the Prussian government, subscribed to the archconservative *Kreuzzeitung*, his wife to the rather more liberal *Germania*. "But what either of them really liked was to read the other's paper" (p. 146).

It was to be Fontane's attitude too, and it is good to read about it again in this fine biography.

JOACHIM REMAK
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MARTIN GREEN. *Mountain of Truth: The Counterculture Begins; Ascona, 1900–1920*. Hanover, N.H.: University Press of New England, for Tufts University Press, Medford, Mass. 1986. Pp. 287. $19.95.

This book narrates one aspect of the so-called German life-reform movement, which sought to regenerate men and women through vegetarianism, antialcoholism, natural healing, land reform, and communal settlements. Life-reform was a response to the rapid growth of cities at the *fin de siècle*, which seemed to endanger bodily and spiritual health by alienating people from nature. The name Monte Verità was given to a hill in the Swiss village of Ascona by vegetarians who built a natural-cure sanitarium there in 1900. For the next twenty years some one thousand people who wanted to opt out of society and point the way to a new paradise lived for various lengths of time in huts or houses on the mountain. Martin Green's book concentrates on three of them: Otto Gross, who tried to integrate psychoanalysis with anarchism and the counterculture, Rudolf von Laban, one of the creators of the modern dance, and Gusto Gräser, guru and prophet, who vitally influenced his close friend Hermann Hesse as well as the German Youth Movement. The first part of the book unravels these three lives; the second is devoted largely to brief sketches of what happened year by year on the mountain, centered on the book's principal characters. The third part of the book attempts to trace the "Asconian Idea" through contemporary art, culture, and politics.

The book focuses on individual personalities, how they lived, interacted, and what they wrote. A concise bond between such men and women is difficult to find; for example, Green's main characters, so we are told, never spoke to each other. Green provides some cohesion to his book through emphasizing the Asconians' joy in their newly found bodily freedom: its movements and erotic potential. Oddly enough, nudism, which was an important component of this freedom, is never mentioned, nor is the mountain's most remarkable structure, Eliasar von Kupfer's *Elisarion* filled with paintings of nude St. Sebastians and androgynous youths. The book fails to encompass the wide variety of Asconian ideals ranging from Gräser's simple-minded rejection of all political thought to Erich Mühsam's anarchism.

The dance best captured the "life-body-movement-expression" (p. 190), which Green considers the essence of the Asconian ideal and which, through the talents of von Laban and his pupil Mary Wigman, became Monte Verità's most successful export. However, the attribution of influence is not always convincing here, for it seems to depend entirely on personal contacts and parallelisms. Almost certainly, writers such as Hesse and even D. H. Lawrence came directly under the spell of Monte Verità, but that Franz Kafka or the painter Emil Nolde knew about the mountain seems doubtful. The specific influence of Ascona is never properly disentangled from the general influence of the life-reform movement. The analysis of the Asconian ideal in politics is the weakest section of the book because the author's single-minded emphasis on personalities neglects the social and historical setting. For example, we read about Gross's interaction with others on the mountain, but we do not learn about his effort to redirect the new science of psychoanalysis from support to opposition to society and its establishment. Moreover, the parallelism between Asconian political attitudes and Ghandi's thought, emphasized here, is much less important than the use made of their visions by *völkisch* thinkers. Green is aware of such cooptation, yet he assigns to the publisher Eugen Diederichs a place on the

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mountain and ignores the basic *völkisch* nature of his thought. Von Laban and Wigman made important contributions to the liturgy of rightist mass movements. They would have liked to contribute to the festivals of the Third Reich, but the regime itself prevented their participation.

Those who want a full account of who was who on Monte Verità will have to turn to the catalogue of the exhibition of 1978, *Monte Verità, Ascona* (1978). Meanwhile, Green's book provides a good and highly readable introduction to a remarkable experiment in which men and women, each in his or her own way, attempted to put into practice their individual visions of a life lived against the dictates of society and the passage of time. Monte Verità should be remembered not solely for its bizarre characters or literary influence but also for ideals that contributed to a counterculture eventually absorbed by society through health-reform and ecological movements. But then, on the mountain itself a luxury hotel has taken the place of the huts of the apostles, and the legacy of the men and women who lived there must now be sought in the very society they sought to escape.

GEORGE L. MOSSE
University of Wisconsin and Hebrew University

MERITH NIEHUSS. *Arbeiterschaft in Krieg und Inflation: Soziale Schichtung und Lage der Arbeiter in Augsburg und Linz 1910 bis 1925.* (Veröffentlichungen der Historischen Kommission zu Berlin, number 59; Beiträge zu Inflation und Wiederaufbau in Deutschland und Europa, 1914–1924, number 3.) New York: Walter de Gruyter. 1985. Pp. xiv, 308. $44.50.

The social and economic dislocations war and hyperinflation caused in Central Europe are receiving renewed attention as fundamental causes of political instability in the 1920s. In this study Merith Niehuss describes the social and demographic impact of rapidly changing economic circumstances between 1914 and 1925 on Augsburg and Linz and thus makes a localized comparison of broader problems in Germany and Austria. Part 1 uses samples drawn from address books to trace social stratification and mobility. The results here are unsurprising: the more dynamic economy of Augsburg permitted greater upward social mobility than did the economy of Linz, although the great majority were immobile through the entire period. Part 2 shows that inflation and the resultant decline in real income of workers caused a variety of indicators of social pathology to increase or peak in the early 1920s: unemployment, alcoholism, murder, robbery, mortality in hospitals, suicide. Unfortunately, the lack of connection between these two sections (mobility and the sample are never mentioned in part 2) and the overabundance of quantitative data prevent this work from rising much above the level of a data handbook.

The number of numbers here is overwhelming: 136 tables and 18 diagrams make for a headlong race through poorly explained data. Numerous tables are mentioned in only one sentence. Many tables are poorly constructed, omitting crucial comparative data or total population figures. Extended discussions of some methodological questions underscore the lack of attention to actual data. After a ten-page comparison of several indices of price inflation, the discussion of the inflation itself covers only two pages (excluding tables). Yet other key methodological questions, especially the use of backwards linkage, are barely addressed.

The brief explanations of tables are frequently misleading: the small difference between Augsburg and Linz in mobility among social classes (16 percent versus 19 percent) is exaggerated (p. 57); twice Niehuss claims that occupational mobility was higher in 1914–21 than in 1921–25 (pp. 77, 85), but the difference can be explained by the differing time spans; the data on page 186 directly contradict the claim that living conditions in Linz were responsible for higher tuberculosis mortality.

Some of the data are fundamentally weak. As the basis for samples tracing social mobility, address books are notoriously unrepresentative of the working class. At the least they need to be constantly rechecked against census data, yet Niehuss bases claims about female occupational distribution on her tiny sample of 166 Augsburg women (pp. 50–51). Her discussion of "structural mobility" (pp. 54–62), occupational change resulting from the evolving distribution of occupations, is based only on the samples of persons linked by address books, which she acknowledges is unrepresentative of total populations.

For specialists in Weimar social history, however, the book offers much of value. Niehuss assembles a wealth of data on the effects of economic dislocation. Her choice of period, spanning the end of the *Kaiserreich* and the early years of Weimar, indicates the prolonged socioeconomic agony that accompanied the introduction of democratic systems in Central Europe. When combined and compared with other local data, Niehuss's statistics could take on greater meaning than they achieve in this unsophisticated work.

STEVE HOCHSTADT
Bates College

WOLFGANG ZORN. *Bayerns Geschichte im 20. Jahrhundert: Von der Monarchie zum Bundesland.* Munich: C. H. Beck. 1986. Pp. 790. DM 78.

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For much of its long and rich history Bavaria was an independent entity and a European power in its own right. Throughout the twentieth century it has been a part of Germany and contributed significantly to that country's political, economic, and cultural evolution. The modern history of Bavaria is, in other words, very much part of the "German problem."

The Bavarians themselves have often seen their history in a somewhat different perspective. Rather like their neighbors to the east, they have tended to regard themselves as the victims rather than as the makers of recent German history. And, again like the Austrians, they have needed to look only to the "Piefkes" in Berlin to find the source of most of their troubles.

Fortunately, Wolfgang Zorn presents Bavaria as a key actor in—not a victim of—recent German history. He examines the contributions (for both good and ill) that Bavarians have made in virtually every aspect of German life, from high-level politics and economics to social trends, military affairs, culture, and technology. We even learn that a *Münchnerin* teamed up with a Saxon to win the Olympic gold medal in pairs figure skating in 1936: significantly enough, they skated to the melody of *Deutschland, Deutschland über Alles.*

Bavaria's role in the Third Reich occupies a central place in Zorn's study, as indeed it must. The author, however, chronicles not just the well-known Munich origins of the Nazi party but also Bavaria's rather less exalted fate during the heyday of the movement when it was reduced to a *Reichsprovinz*, Hitler having moved from Munich's "Brown House" to the Reich Chancellory in rival Berlin. But, lest this provide fuel for the victim theory of Bavarian history, Zorn makes clear that, if Bavaria produced its share of anti-Nazi resisters (most notably the "White Rose" group at the University of Munich), it also continued to supply its quota of party ideologues and SS and SA thugs. Munich, the only German city to boast a swastika in its coat-of-arms, remained proud of its title "Hauptstadt der Bewegung" just as Nuremberg gloried in its role of host city to the Nazi party rallies and Bayreuth in its possession of the Richard Wagner *Festspielhaus*, which Hitler turned into Nazism's court theater. Only when Allied bombs began reducing Bavarian cities and towns to rubble did their inhabitants recall their affinity with Austria and insist on depicting the war as the latest in a long line of calamities visited on them by Prussian barbarism. (Since the war they have never forgiven the Austrians for successfully marketing this interpretation of Central European history whereas they could not.)

Students of the postwar era will welcome Zorn's substantial treatment of Bavarian developments during the Allied occupation and first two decades of the Federal Republic. If there has been any revenge for 1866 in German history it occurred during this latter period, when the *Freistaat Bayern* led a resurgent south Germany to economic superiority over the north. In politics Franz Josef Strauss, the "uncrowned King of Bavaria," emerged as a key figure in federal German politics. Culturally, of course, Munich prospered at the expense of divided and isolated Berlin; although it never regained the cultural preeminence it enjoyed at the turn of the century, it became an important center for publishing, music, and (especially) film. Even in military affairs, long a bastion of the Prussians, Bavaria quickly assumed a leading role. A Bavarian, Richard Jaeger, chaired the Security Committee in the Bundestag; Josef Kammhuber, another Bavarian, became the new Luftwaffe's first inspector; and Strauss himself became defense minister in 1956, holding that office until the *Spiegel* affair of 1962 drove him (temporarily) out of Bonn.

Zorn's study not only examines Bavaria's relations with the rest of Germany and with Europe but also chronicles developments within that state. He provides a great deal of information about Bavarian Catholicism and Franconian Protestantism and about social and cultural life not just in Munich but also in smaller Bavarian cities such as Augsburg, Nuremberg, and Regensburg. We learn much about marriages and social connections within the Bavarian nobility. The state's rise to industrial prominence is especially well treated as are important developments in agriculture. The author does not ignore the somewhat separate history of the Bavarian Palatinate. Discussion of these matters is no doubt Zorn's greatest service, for single-volume histories of modern Bavaria rarely go much beyond the perspective that might be gained by climbing the towers of the *Frauenkirche*.

But breadth of scope and richness of detail are also a problem here because Zorn has failed to present his abundant material as effectively as he might have. The book would certainly have been strengthened by a more rigorous thematic organization or perhaps by the addition of an introductory chapter setting out the central themes. As it stands, the reader must have considerable orienteering skill to negotiate this twisting and boggy path from the *Prinzregentenzeit* to the end of the Adenauer era.

DAVID CLAY LARGE
Montana State University

DIETRICH ORLOW. *Weimar Prussia, 1918–1925: The Unlikely Rock of Democracy.* Pittsburgh: University of Pittsburgh Press. 1986. Pp. xii, 363. $32.95.

Dietrich Orlow, known for his detailed history of the Nazi party, deals here with the structure and func-

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tioning of the Prussian state government in the critical early years of the Weimar Republic. His book appears at the same time as the lengthier treatment of Horst Möller, *Parlamentarismus in Preussen* (reviewed in the *AHR* 91 [1986]: 687–88). The two books are very different, but they reach much the same conclusions: that Prussia's democratic government was far more successful than the Reich's and that it served as a worthy prototype of the Federal Republic of today. Orlow's study is based on extensive use of archival and parliamentary collections and effectively draws on a number of memoirs and diaries. Sometimes, however, it betrays a certain lack of broad background in German history: for example, in translating the name of Prussia's core province as the "Marquis of Brandenburg" (p. 91) and in referring to the Christian Social movement of "Paul" Stöcker (p. 121).

The Kapp Putsch of 1920 is the turning point that divides Orlow's discussion of political parties, the formation of cabinets, and problems of personnel and administration. He finds that the defeat of the putsch encouraged the democrats and, what is most important, persuaded the leadership of the Prussian branch of the German People's party to abandon its support of the German Nationalists and agree to participate for three years in a cabinet of the Grand Coalition. This in turn strengthened the state government during the crises of political assassinations and the French occupation of the Ruhr. The relative reasonableness of the People's party in Prussia was matched by the cooperation of the other key parties, the Social Democratic party, led in Prussia by its moderate right wing rather than by the left as in the Reich, and the Center party, led in Prussia by its left wing rather than the right as in the Reich. In the author's words, "Pragmatists rather than ideologues dominated the state's political scene" (p. 8).

Another factor in Prussia's success was that the cabinet ministers were professional party politicians from the Landtag rather than "unpolitical" figures such as Wilhelm Cuno and Hans Luther in the Reich government. Unaccountably, Orlow does not stress a related factor that is central to Möller's argument: that in Prussia the Landtag had the sole constitutional responsibility for choosing the minister-president and for resolving party conflicts, whereas, in the Reich, the decisive role of the president contributed to the fatal weakness of the Reichstag. Thus, that Prussia "enjoyed consistent political stability under the same political system" as the Reich is not strictly true (p. 8). Furthermore, Orlow's descriptions of endless party bickering and agonizing over the formation of coalitions demonstrate that Prussia was "stable" merely in a comparative sense. He points with pride to "only" six ministries and four prime ministers in the state in seven years and dismisses the two ministries of Adam Stegerwald and Wilhelm Marx as brief transition governments; he forgets that they were "transitional" only in hindsight, not in intention. The state was a better "rock of democracy" in the later period from 1925 to 1932.

Orlow is not completely convincing in his insistence that the Weimar leaders of Prussia were inspired by the state's special historical identity and shared purpose (which he labels, oddly, "étatisme"). He has shown, however, that the existence of a well-governed and relatively stable Prussia was crucial in enabling the Weimar Republic to survive its first six years, and he has provided a useful and worthwhile counterpoint to its history at the national level.

ELLEN L. EVANS
Georgia State University

ERIC G. REICHE. *The Development of the SA in Nürnberg, 1922–34*. New York: Cambridge University Press. 1986. Pp. xviii, 314.

Researchers who have dug down into the mire of local and regional Nazi party organizations owe a debt to Eric G. Reiche, whose doctoral thesis of 1972 on the Nuremberg SA was the first detailed local study of the storm troopers. Now, after an unfortunately long interval, Reiche's study has been published. What does it offer?

Reiche presents a blow-by-blow account of the SA in Nuremberg from its beginnings until the purge of 1934 and provides detailed tables outlining its growth and social composition on the basis of records of 358 former storm troopers. It contains few surprises: the Nuremberg SA was involved in political violence similar to that throughout Germany as the Weimar Republic collapsed; it played a key role in recruiting activists to the Nazi movement; it contained a fair number of workers but "appealed to virtually all sectors of the city's male population" (p. 229)—including professional people; and it was involved in a series of conflicts with the Nazi party organization (culminating in the "Stegmann revolt" shortly before Hitler came to power).

Reiche has produced a well-crafted monograph, but the story he tells already is quite familiar. Yet, in many ways, Nuremberg offers a particularly fascinating place in which to study violent Nazi politics. It was an important industrial center; it was a major stronghold of the Social Democrats; its politics (as Reiche notes in the introduction) were colored by antipathy toward Munich (the seat of both the Bavarian government and the Nazi party headquarters); and it was Julius Streicher's stomping ground. Although the Nazis and Communists frequently fought in the streets, the most inviting targets for Nazi attacks were the Social Democrats—with their

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impressive organizational network of newspapers, trade unions, and party cells. The Social Democratic party was identified with the republic, with authority, with the "system"—in few places more so than in Nuremberg. Did this give a peculiar character to SA activities and recruitment in the city or to the relations of SA men with their neighbors and colleagues? How did the Nazis organize on the streets and in neighborhoods within a predominantly Social Democratic political environment? Unfortunately, such questions remain unasked and unanswered.

One problem with the book is that Reiche tends to take his sources a bit too much at face value. He informs us in detail of the expressed sentiments of SA leaders and men, for example, their "feeling of superiority over the party" (p. 134) and their anger at Streicher's behavior and corruption. But why did SA leaders use the language they did, and to what extent did they speak for the membership? We learn a lot, for example, about the leadership's intentions for the special SA formations, but what did these formations actually do? Why did the young men who became storm troopers want to prove their toughness in this fashion and play soldier? And what does all the tabulated data actually tell us about why people involved themselves in this peculiar violent brand of politics? On this last point, Reiche is wisely cautious, but at times his caution is overwhelming. The result is a book that is strong on description, but readers looking for new insights into the rise of the Nazi movement or the nature of political violence in Weimar Germany will be disappointed.

RICHARD BESSEL
Open University

ALICE GALLIN. *Midwives to Nazism: University Professors in Weimar Germany, 1925–1933*. Macon, Ga.: Mercer University Press. 1986. Pp. viii, 134. $16.95.

If you missed the debate fifteen years ago about the role of German university professors as precursors of Nazism, you can now read a potted version of several of the books (if not of the debate itself). A conversation with Fritz Ringer in 1971 spurred Alice Gallin to take up her pen, and the journalist Hans Peter Bleuel suggested the term "midwives" in her title. This study has more the character of a brief essay than of a work of original research. The only justification for such a publication would be if it brought a fresh perspective to the question. It does not. In fact much of the recent literature is not even mentioned—80 percent of the articles cited in the bibliography are over twenty years old.

Why did professors not halt the rise of Hitler? Gallin seeks the answer in professorial thought and university structure in the Weimar period. She tries to draw up a profile of German academic values by examining the narrow base of public speeches of full professors at the universities of Bavaria and Württemberg. A lack of precision in their usage of terms such as "Reich," "Führer," and "Kulturmission" draws her criticism. Gallin finds the same language in the later speeches of Hitler and Goebbels. Post hoc, ergo propter hoc?

The author's insistence that this common rhetoric is the key to understanding the smooth *Gleichschaltung* of universities in 1933 represents an overly intellectualized interpretation that is too far removed from the real world. She accuses the professors of seeing no need to protest the removal of their Jewish colleagues. Her main documentary sources for gauging the atmosphere of the university, however, are the contemporary reports of U.S. State Department officials to Washington, which must be second- or third-hand accounts at best. Ideological concurrence was not the whole story. Physical violence threatened German professors, and they acquiesced in the Civil Service Law partly on the understanding that indiscriminate action and firings would be brought under control by the government.

In a number of other instances Gallin appears insufficiently conversant with the literature to interpret matters correctly. She asserts that Kurt Huber, who was executed with the White Rose Resistance group, joined the Nazi party in 1940 "out of fear of communism." Actually his wife signed him up without his knowledge in a (successful) attempt to increase his meager salary, as Christian Petry revealed in 1968. Gallin's interview in 1972 with the "distinguished-looking . . . gracious, thoughtful" Albert Speer in his "charming family home" evidently dulled her critical faculties. Speer told her that Hitler often remarked that he had no problem obtaining the cooperation of the universities. Yet Hitler both publicly and privately expressed his frustration with academics. If Hitler really did make the remark (which is open to doubt), then its context would have to be sought. Gallin should not have relied on this comment to prove Hitler's overall satisfaction with the professoriate or the identification of academics with the National Socialist movement. That has long been known not to have been the case.

GEOFFREY J. GILES
University of Florida

ANNA BRAMWELL. *Blood and Soil: Richard Walther Darré and Hitler's "Green Party."* Bourne End, Buckinghamshire: Kensal. 1985. Pp. viii, 288. £12.95.

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Born to German parents in Argentina in 1895, Richard Walther Darré became the leading Nazi spokesman for a "blood and soil" agrarian ideology. After successfully coordinating Nazi infiltration of traditional German farmers' organizations between 1930 and 1933, Darré assumed a number of important positions in the Third Reich, ranging from minister of food and agriculture to national peasant leader and head of the SS Race and Settlement Office. Although Darré's role in Nazi agrarian politics has attracted considerable attention from such scholars as Horst Gies, Gustavo Corni, and J. E. Farquharson, Anna Bramwell has written the first comprehensive, scholarly biography of Darré. Bramwell effectively used the papers of Darré and Herbert Backe and extracts from the diary of Darré, which was destroyed in the late 1960s. The only major weakness in Bramwell's research was her failure to consult the SS files in the Berlin Document Center.

This is a thorough, if at times too sympathetic, survey of Darré's ideology and political activities. Darré, a trained agronomist with practical experience in the Baltic region, wanted to protect the Nordic peasantry, the life source of the Germanic nation, by ensuring the economic viability of peasant farming and encouraging a "eugenically-oriented racialism" (p. 201). Bramwell distinguishes between Darré, the "racial tribalist," and Heinrich Himmler, the "imperialist with romantic racial overtones" (p. 130), although she admits that Himmler implemented many of Darré's ideas in a perverted way. Also, Darré supported the resettlement of "German lands" in the Baltic region and in Poland, but he opposed Hitler's invasion of Russia. According to the author, Darré's power began to wane after 1936 because he remained true to his original ideas. In 1938 he was removed as head of the SS Race and Settlement Office, and in 1942 he was dismissed as minister of agriculture after having lost most of his real authority to Backe.

Unfortunately, the book is marred by a number of minor problems. Carl Goerdeler is described as the former mayor of Danzig instead of Leipzig; Günther Pancke, Darré's successor as head of the SS Race and Settlement Office, is referred to as Gustav Pancke; and the SS Race and Settlement Office is discounted as a "cardboard edifice" without adequate explanation of its organization. Contemporary reports of the Sicherheitsdienst also make clear that Bramwell is incorrect in asserting that the official explanation of Darré's dismissal in 1942, as merely a temporary leave because of illness, was widely accepted by the public. More serious are the author's contentions that between 1919 and 1931 "agrarian ideology seems not to have touched Himmler's political life" (p. 133) and that "Hitler's need for the rural vote was based on a shortfall in the 1930 vote" (p. 83). In fact, Himmler helped organize Nazi rural propaganda before 1928, and the "shortfall" occurred in 1928, not 1930. Finally, are we really to believe that without Darré "the ecological movement would have perished in his time and place" (p. 200)?

JOHNPETER HORST GRILL
Mississippi State University

FRANCIS R. NICOSIA. *The Third Reich and the Palestine Question*. Austin: University of Texas Press. 1985. Pp. xiv, 319. $35.00.

Few countries during the early twentieth century concerned German policy makers less than Palestine. First an Ottoman outpost and after 1919 a British mandate, Palestine was a mosquito- and disease-ridden swamp with approximately two thousand German Christians living amiably among their Jewish and Arab neighbors. During the early years their influence extended to a few scattered German schools, hospitals, and orphanages and ten Protestant Temple Society settlements. German imports from Palestine were infinitesimal. Berlin had no territorial interests in the area and, in fact, had long conceded the Middle East to British control. What little policy existed during the Weimar years was generally in support of the status quo and mildly sympathetic toward the embryonic Zionist movement. Like most Europeans, the Germans viewed the Arabs with contempt.

With the appearance of the Third Reich, policy toward Palestine—still marginal in German diplomacy—was governed by three factors. The first was the desire for British friendship and hence the continued policy of acquiescence to the status quo in Palestine. Until the Munich Crisis of 1938 shattered any future cooperation between Germany and England, Hitler carefully refrained from any official activity that could be interpreted as hostile to Great Britain. The second factor in Hitler's contradictory policy was the view that the Jewish problem could be solved through emigration, with Palestine as the most convenient destination. It was after failing to encourage such emigration that the Nazis embraced mass murder. The last factor was Hitler's general disdain for the Arabs. In all three areas, the Nazis found it most appropriate to turn for help, astonishingly, to the tiny Zionist movement in Germany.

Their cooperation began as early as August 1933 when Berlin signed a trade agreement, the *Ha'avara* or Transfer Agreement, with the Anglo-Palestine Bank. In a complicated arrangement Jews willing to emigrate to Palestine were permitted to transfer twenty thousand dollars in specific trade goods, which were to be resold to facilitate settlement in their new homeland. Thus began a curious rela-

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tionship in which the Nazis provided preferential treatment for German Zionists over other German Jewish groups, encouraged programs to retrain German emigrants bound for Palestine, and even participated in illegal efforts to smuggle refugees into Palestine after Britain closed the gates.

Germany's interest in Palestine, admittedly never of great importance, had its effect on various other issues as well. For example, since the five hundred thousand Jews living in Germany in 1933 (constituting a scant 0.76 percent of the total population) were overwhelmingly non- or anti-Zionist, the government's decision to work with the Zionists forced many to redefine their Judaism. Without question, the Nazis significantly bolstered the tiny Zionist movement, and one can marvel at the irony that saw the Nazis support the mass emigration of Jews to Palestine without drawing the logical conclusion that such increased immigration would inevitably strengthen the nucleus from which a Jewish state would emerge in due course. Germany's relations with Britain on the issue of Palestine were no less complicated than its dealings with the Arabs.

Francis R. Nicosia has written the definitive study of this fascinating but thoroughly minor epoch in the histories of the participants. It is a masterful examination of every interwoven thread in the complicated tapestry of Nazi Germany's relations with the Middle East, as well as with Great Britain and the Zionist movement—a mastery that the author immodestly trumpets at length in his preface. Still, Nicosia has written an impressive if overblown study; indeed, a full third of the manuscript, one hundred pages, is devoted to appendixes, elaborate footnotes citing records from a dozen archives on three continents, and a twenty-page bibliography (the only notable omissions being the works of Solomon Colodner and the prominent East German journalist, Klaus Polkehn). If the sheer weight of the information and the painstaking research does not convince the reader of the book's importance, perhaps the preface will.

ARNOLD KRAMMER
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MANFRED KÖHLER. *Die Volksschule Harsum im Dritten Reich: Widerstand und Anpassung einer katholischen Dorfschule*. (Beiträge zur historischen Bildungsforschung, number 2.) Hildesheim: August Lax. 1985. Pp. 214. DM 40.

This is a history of the public elementary school of Harsum, a village about six kilometers north of Hildesheim and now in the *Land* of Lower Saxony in West Germany. Originally part of the territorial possessions of the bishop of Hildesheim, after the secularization of 1802–03 it became part of Hannover and in 1866 was incorporated into Prussia. The village was and is overwhelmingly Catholic; its school was organized on a Catholic confessional basis under the bishop, and its nature and orientation have not changed. Manfred Köhler has written a book designed for local readers; he provides details of school construction, biographical notes on teachers, and an unpaged annex of pictures of various school classes, the faculty, facsimiles of assignments, and other school matters.

Although the book is local history and brings no startling new material, it presents interesting details on conditions before and after the advent of Hitler. In a totalitarian state anything in opposition is in a sense resistance (*Widerstand*), and, outwardly, not much of this existed in Harsum. The word *Anpassung* (adaptation, accommodation) in the title really describes the school's response to the Hitler era. Here, it is excellent. The teachers were all practicing Catholics, but the male members of the faculty joined the party, only one of them with any enthusiasm. Emil Dorpmund, who headed the school from 1930 to 1945 and did not join the party until 1937, was most adept at steering a safe course for the school. Radios were set up so the students could listen to Hitler's speeches and the Hitler salute was used. The school complied with ceremonial raising of flags, observed decreed patriotic holidays, used new reading and mathematics textbooks introduced in 1935–37, enlisted students to collect old clothes, metals, and medicinal herbs during the war, and observed other party programs. The Catholic spirit and orientation of the school and its ties to the church were not changed. Religious instruction continued as before, and the conversion from a confessional to an interdenominational school (*Gemeinschaftsschule*) was stalled until May 1, 1942, although that program was launched in 1935. A few Protestant children always attended the school, the numbers varying from nine to fifteen during the 1930s. Little is said about these pupils although Protestant religious instruction for them began in 1904. Restrictions were placed on Catholic organizations after the conclusion of the concordat in 1933, but no great controversies arose in Harsum. Apparently, no attempts were made to remove crucifixes and religious pictures from the classrooms. Köhler also examines denazification procedures, particularly the case of Dorpmund. Parents voted 97.1 percent for the reestablishment of a confessional school after the war. Clearly, the loyalty of the inhabitants to their church and their ancestral piety prevented great inroads on the conduct of the school. The author stresses that instruction remained much as it had always been—*Alles blieb beim alten*. The book is well documented; there is a detailed table of con-

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tents, a bibliography, a useful two-page table of abbreviations, but, alas, no index.

ERNST C. HELMREICH
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RUTH BETTINA BIRN. *Die Höheren SS- und Polizeiführer: Himmlers Vertreter im Reich und in den besetzten Gebieten*. Düsseldorf: Droste. 1986. Pp. x, 430. DM 48.

Ruth Bettina Birn has produced a masterly study of the office of higher SS and police leader and those who held this position in the period 1937–45. At the maximum there were thirty-seven positions of widely divergent power and function. Twenty-six men held just one such position; two men held four different higher SS- and police positions serially; three held three different posts between 1938 and 1944. Ten moved from one position to another. Moreover, some of the names are familiar in other SS roles: Karl Wolff, Ernst Kaltenbrunner, Richard Hildebrand, Otto Hofmann, Erich von dem Bach-Zelewski. So not only were the positions vastly different—internal Reich, Austria, Bohemia-Moravia, annexed eastern territories, General Government, Army Group A, Adriatic Littoral, Denmark, Serbia, military commander of Belgium–Northern France, and *z.b.V.* (on special assignment)—but the importance and employment of the men themselves were markedly different. Birn inclines toward systematization and generalization and strongly favors an "intentional" interpretation, with respect both to Himmler and Hitler and to the SS. Playing down improvisation and accident, the author stresses the way the conflict motif in National Socialism, and especially in the SS, was essential to its world view and was a method of human selection. Provided conflict led to greater destructive efficiency in the war against designated enemies (Jews, Slavs, "criminals," shirkers, sexual and mental deviants, partisans), the conflict always was backed by Himmler, and the victors were the most ruthless. Birn is quite conscious of the centrifugal tendencies in the SS and in National Socialism: the higher SS and police leaders owed their rise and power to the ever-unfolding chaos and internecine struggle, especially from 1941 to 1944.

This book has many strengths. It is clearly organized and written in a straightforward style. After an introductory survey of origins, legal and extralegal competencies, territorial structure, and finance, Birn provides an excellent functional analysis of the relationship of these factors not only to the SS and police but also to the major authorities of the Reich. If anyone can find rationality and logic in the creation of Heinrich Himmler, it is Birn. This is not intended ironically: she successfully winnows out the purposive from the improvised. Next, the author groups the various positions into a typology: colonizers of the east, *grossdeutsch* politicians, military administrators, foreign policy strategists, stiffeners in crisis, and home front guards. This section is much less analytical but, like the earlier parts, beautifully researched and illustrated. Finally, biographical and sociological profiles supply some important insights. These men were not "characteristic" Nazis: they ranged from blue blood to blue collar; they were not especially from the lower middle class, nor were they, in spite of their high SS rank (generals), very well educated. She finds their common denominator in their *Weltanschauung*, which they took very seriously. Technocrats? Yes, but they were also true believers. The higher SS and police leaders were not the exclusive executors of the SS: the Main Office chiefs formed a separate fraternity (although a few of the leaders had held this position); the concentration camp empire, the Waffen SS, and the Replacement Army after July 20, 1944, remained outside their grasp.

Birn has written an important study. All students of the SS will have to consult it, and students of National Socialism generally will gain from her perceptive handling of Nazi faith and style. The typescript format is a bit hard to read, but it is apparently without errors.

ROBERT KOEHL
University of Wisconsin

GERHARD HIRSCHFELD, editor. *The Policies of Genocide: Jews and Soviet Prisoners of War in Nazi Germany*. Foreword by WOLFGANG J. MOMMSEN. Boston: Allen and Unwin, for the German Historical Institute, London. 1986. Pp. xiii, 172. Cloth $22.95, paper $9.95.

This important, controversial work developed from seminars held at the German Historical Institute in London. Except for Falk Pingel's sensitive essay on resistance in the camps, it focuses on how the Holocaust originated and the participation by various groups within Germany. These essays, edited by Gerhard Hirschfeld, represent the scholarship of a generation of German historians who did not personally experience National Socialism and World War II.

Current scholarship on the Holocaust divides between "intentionalists" and "structuralists." Intentionalists see Hitler's ideological intent as decisive; structuralists minimize his role and emphasize the piecemeal emergence of the "Final Solution."

The essays by Christian Streit and Jürgen Förster reject attempts to draw a sharp dividing line between the activities of the SS and those of the army. The *Wehrmacht* accepted Hitler's definition of the

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war in Russia as an ideological one and shared his goal of the permanent elimination of Russia and bolshevism. It willingly carried out the infamous commissar order. Its participation in the killing of Russian prisoners of war (which Streit has documented in a previous book) consisted of handing over some five to six hundred thousand selected Russian POWs to the SS (commissars and functionaries, agitators, fanatical Communists, and all Jews) as well as the systematic starvation of prisoners under their control (out of 5.7 million, 3.3 million perished). This has received scant attention in historiography of the Holocaust or World War II.

Lothar Kettenacker addresses the issue of whether the mass killings were due to "a single, incomprehensible fanatical will [of Hitler] . . . or did the Jews get caught up in the machinery of a system of power . . . that had gone out of control" (p. 73)? He notes that individuals sought a rational justification for their murderous acts. Himmler declared that the Jewish question was "a considerable factor in the internal security of the Reich and Europe," and Hans Frank observed that if the Jews had not been eliminated it would have been "like sitting on a volcano, which could have erupted at any minute" (p. 88). Kettenacker also refers to the "confusion among the traditional conservative elites about the priorities of justice and legality" (p. 88).

Hans Mommsen, at the center of the current historiographical controversy, rejects the existence of an order from Hitler for extermination (for which Kettenacker cites some evidence). "The idea that Hitler set the genocide policy in motion by means of a direct instruction can be completely rejected" (p. 127). Mommsen uses the absence of such an order to explain the mass repression of knowledge of genocide by individuals (such as Albert Speer) in influential positions. If not Hitler, then who? "The utopian dream of exterminating the Jews could become reality only in the half-light of unclear orders and ideological fanaticism. . . . The process developed its own internal dynamic" (pp. 127–28). This, Mommsen argues, makes it impossible to assign responsibility to a few individuals.

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EARL R. BECK. *Under the Bombs: The German Home Front, 1942–1945*. Lexington: University Press of Kentucky. 1986. Pp. xi, 252.

The history of war provides fascinating insights into the popular concept of armed conflict. Accounts of national violence often present tales of individual heroism, collective success, and official rationalization. Such presentations neglect the human realities of modern struggles: the aimless slaughter of innocents, the constant battle for survival, the omnipresent controls of governments, and so on. Hiding or avoiding such realities contributes to the continuance and acceptance of violence as a viable aspect of the human condition.

Earl R. Beck's useful volume provides an insightful description of Germans within the construct of a dreadful war. His study seeks a balance between the large-scale deteriorating conduct of war, the governmental efforts for control of the home front, and the individual struggles for survival. He takes no moral position concerning the issues because judgment is not part of his purpose. He wants understanding and awareness of individuals caught up in the holocaust of war—irrespective of their personal guilt or innocence. His narrative provides a dispassionate account of the circumstances in Germany and the hapless decline into the chaos of 1945. It was a terrifying descent filled with fear and, ultimately, horror.

Relying on a careful scrutiny of published materials and some documentary sources, Beck provides a well-written, well-organized account, which reflects his professional skill and long experience. He adds an excellent portfolio of pictures as a complement to the text. In this sense his effort merits approbation for intent, readability, and conciseness.

As long as he keeps to the theme stated in his title, Beck does well. When he describes the more complex national efforts, both in the realm of military operations and political controls, he is less successful. The length of his book precludes careful attention to these areas, and his basic purposes make the meld between them and the individual realities of the home front awkward and uneven.

With that caveat aside, one must compliment Beck for his sensitivity. His book provides excellent reading and learning. One can recommend it as a powerful study.

CHARLES BURDICK
San Jose State University

RONA GOFFEN. *Piety and Patronage in Renaissance Venice: Bellini, Titian, and the Franciscans*. New Haven: Yale University Press. 1986. Pp. xvii, 285. $40.00.

Rona Goffen attempts to show how Franciscan piety and lay patronage affected the creation of three masterpieces of Venetian Renaissance painting. She brings to the method of contextual art history an admirable range of documentary, theological, and visual material. The result is a provocative, if uneven, assessment of art as an expression of cultural values in Renaissance society.

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Goffen includes five chapters with two appendixes and illustrates the text with four color plates and fifty-nine black-and-white photographs of varied quality. She begins with the history of the Conventual Franciscan church of Santa Maria Gloriosa dei Frari, where all three altar paintings remain in their original locations. Goffen proposes that this fourteenth-century priory serves as a microcosm for Venice's art, the Franciscan order, and the relationship of both to the state.

The next three chapters examine each painting: Giovanni Bellini's Frari triptych (1488), Titian's *Assumption of the Virgin* (ca. 1516), and his *Pesaro Madonna* (ca. 1519). She argues that the altarpieces interrelate on varied levels. What is most important, she believes that each celebrates the Franciscan devotion to the cult of the Immaculate Conception of the Virgin encouraged by the Franciscan pope Sixtus IV (1471–84). She also argues that these paintings reveal how the patrons linked their careers to the civic values of Marian devotion in Venice. The final chapter, in fact, problematically suggests that the Venetians celebrated the specific Marian cult of the Immaculata.

The book is most convincing when Goffen discusses the Pesaro family's art patronage. Finally, we can appreciate Bellini's altarpiece as the center of the combined sacristy-funerary chapel decorations, which, unexpectedly, commemorate a woman, Franceschina Tron Pesaro. Commissioned by her four sons, the painting shows their name saints flanking the Virgin and Child and includes inscriptions that apparently celebrate the feast of the Immaculate Conception. Goffen persuasively identifies the family portraits for Titian's *Pesaro Madonna*. In perhaps the book's best section she demonstrates that the rivalry between the family's admirals, Jacopo and his cousin, Benedetto, affected the art they ordered to memorialize their naval triumphs for the Serenissima.

The book is least persuasive when it argues for the specific meaning of the Immaculate Conception on the basis of paintings that offer equally plausible support for the prevalent celebration of Mary as *mediatrix*. Although the animosity between Franciscans and Dominicans about Mary's immaculate conception poisoned the atmosphere of the mendicant community, insufficient visual evidence remains for this period to claim that their respective positions determined artists' representations of the Assumption. Titian's *Assumption* distinguishes itself most by its spectacular color and internal light not by its approximation to a fifteenth-century sermon by Lorenzo Giustiniani. Moreover, Filippino Lippi's *Assumption* in the Dominican church of Santa Maria sopra Minerva in Rome does not, like Titian's painting, allude to both the Coronation and the Assumption. Filippino's angel carries a tamborine, not a crown. The Dominican theologian Vincenzo Bandello de Castronovo, by the way, did not spell his name Bandinello.

Although this book does little to suggest the artists' individuality and creative power, it helpfully places their work into a richly documented context of Franciscan piety and both secular and religious patronage in Renaissance Venice.

GAIL L. GEIGER
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AURELIO CERNIGLIARO. *Sovranità e feudo nel regno di Napoli, 1505–1557*. In two volumes. (Storia e Diritto, number 11.) Naples: Jovene, 1983. Pp. xxxix, 386; 390–1070.

Through his study of the kingdom of Naples in the first half of the sixteenth century, Aurelio Cernigliaro examines one of the main problems in the history of the modern state: the final outcome of centuries of strife for political and social power between feudatories and the crown. In Naples an unusual solution was adopted. After the kingdom was annexed to the Spanish crowns, the king of Naples had access to vastly superior military forces and possessed diplomatic and political advantages that gave him independence from feudal agreement and internal support.

The originality of Cernigliaro's contribution lies not so much in his study of the problem itself as in his minute analysis of the juridical and political methods the monarchy employed to gain primacy and subdue feudalism. But this analysis does not aim to build any particular theoretical or systematic pattern of the growth of the modern state. Juridical history, as seen by Cernigliaro and the Neapolitan school headed by Raffaele Ajello, is a history of social struggles, political conflicts, opposition of ideas, and contrasts between private or institutional elites; the law only reflects the tensions and the balance between political and social process.

In the kingdom of Naples the outcome was the preeminence of the *ministerium* or *togati*, that is, the clerks of the new state resulting from the victory of the crown. This development had nothing to do with the classic alliance between the monarchy and the bourgeoisie, and in this instance the mechanism of bureaucracy was not a sociological definition or delimitation. The ministerial structure of government was the final stage of the ancient *curia regis*, now transformed into a social milieu of jurists, *gens de lettres*, and noblemen, whose familiarity with the technical formulas that were frequently employed in the new political and social situation made them indispensable to the king. Cernigliaro indicates that the reform of the *Consiglio Collaterale*, the council set up by Ferdinand the Catholic in 1507 as the main

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support of the viceroy, was the crucial element in the formation of a new political leadership. Under Charles V the *milites* of the *Collaterale* were excluded from the council except when military matters were under discussion.

The course of events was extremely complex and cannot be summed up as a simple antithesis between the king and his vassals. In 1508 and 1547 the Spanish kings unsuccessfully tried to introduce the Spanish Inquisition into the kingdom of Naples, even though in 1547 the viceroy, Pedro de Toledo, seized the occasion to weaken the nobles' position in the city's main governing body. Cernigliaro's thesis regarding the events of 1547 is original, but he also provides evidence of the close bonds between barons and *togati* and, above all, of the persistence of strong feudal power in the provinces. One of the best aspects of Cernigliaro's study is the description of the way in which the *togati*, owing to their position as *extra* or *super partes* as the king's *longa manus*, assumed an intermediary role between the barons and the new balance of power.

In Cernigliaro's view the parliament was not only a means of feudal resistance and reinforcement but also a crucial and delicate point in the whole system. According to the new evidence examined by Cernigliaro, the parliamentary sessions in 1556 and 1557 reveal that these years were crucial in the establishment of new roles and status. The real power behind royal despotism was the *togati*. The dismissal of Toledo after the failure of his attempt, in 1547, to establish the king's personal rule by introducing the Spanish Inquisition reveals the real course of events: the baronage was strong only when it was allied with the *togati*, which was the case in 1547.

The estates had not yet emerged, but this development definitely marks their beginning. In Cernigliaro's book the importance of this social pattern and its relationship to the ministers in power do not emerge very clearly, but the configuration of the feud in the new regime is persuasively illustrated by an original analysis of feudal investitures. The formulas employed in drawing up these deeds—almost always completely neglected by historians—are impressive and enlightening. The concept of power and its social articulation are clearly seen in relation to the new situation of the kingdom.

The same can be said for another neglected deed contained in Neapolitan public law: the *capitulationes* between the king and parliament and other institutions, which reveal—particularly under Philip II—a reciprocally advantageous agreement.

On one hand, royal absolutism was constantly confirmed and the viceroy's power was strengthened to the advantage of the *togati*. On the other hand, the *togati* and the other orders that made up the kingdom supported a tendency toward autonomy from Spain and in so doing achieved an internal equilibrium, even during their frequent conflicts. In this way dependency on a foreign power was counterbalanced (although obviously not reversed) by the vivacious and complex traits of a country rich in cultural and human vigor.

As the reader may easily conclude, the beginning of the *respublica togatorum* in sixteenth-century Naples reveals a tendency to assume the aspect of a conservative regime.

In Cernigliaro's view, the ennoblement of the *togati*, which became an accepted custom, is the best evidence of their success, sanctioned by their penetration into institutions that were once the strongholds of the artistocracy. In this case a less optimistic judgment may be necessary to discern the sense not only of Neapolitan modern history but of that of the whole of Europe, which Cernigliaro's book invites us to do. The great importance of this book lies both in its several new ideas, based on the author's rather unusual exploration of documents, and in his evident interest in the development of the modern state in Europe.

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FRANCESCO BENIGNO. *Una casa, una terra: Ricerche su Pacco, paese nuovo nella Sicilia del Sei e Settecento.* Catania: Cooperativa Universitaria Editrice Catanese di Magistero. 1985. Pp. 215.

One of the more original responses to the increasing difficulties experienced by the seigneurial economy toward the close of the long "Mediterranean century" developed in Sicily. Known as internal "colonization," this movement saw the foundation by aristocrats of new "feudal villages" devoted largely to the cultivation of cereals, made increasingly profitable thanks to recent population growth both in Sicily and overseas. One such settlement was Paceco, founded in the early seventeenth century by Don Placido Fardella, an enterprising and well-connected member of a rapidly rising noble family from the nearby city of Trapani. Following the initial construction of housing and public buildings by Fardella's agents, village land was divided into two categories: a latifundium subjected to direct seigneurial control, which included most of the arable land and almost all pasturage, and a lesser group of minifundia, or small peasant holdings leased through emphyteusis or quitrents. Success in establishing a colony depended on achieving a delicate balance between offering incentives sufficiently enticing to lure new inhabitants and levying enough dues to assure recovery of the initial investment as well as ongoing seigneurial profits. Unfortunately

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for Fardella and his successors, the colony of Paceco apparently failed to live up to expectations.

The bulk of Francesco Benigno's study consists of a brisk but thorough analysis of local population trends and property structures. Comparison of the *riveli*, the periodic censuses long a favored source of students of Sicilian demography and economy, reveals growing stratification within Paceco during the seventeenth and eighteenth centuries. A widening gap separated a small nucleus of *burgisi*, or middling property owners, from a much larger stratum of inhabitants possessing little or no fixed wealth who were obliged to seek day labor on either the baronial demesne or the holdings of their more prosperous neighbors. Benigno also argues that the fundamental unit of production and reproduction in the village was the nuclear family, characterized by a relatively low age at first marriage and neolocal residence. In short, although landholding patterns in Paceco conformed from an early date to a generalized trend toward the markedly unequal distribution of property, local family and household structures differed dramatically from the "Mediterranean family" model beloved by earlier generations of historians and ethnographers.

Internal colonization has long interested historians of early modern Sicily. The pioneering phase of study, which centered largely around questions of feudal law and the impact of these aristocratic initiatives on the relations between the parliamentary estates, has now given way to a growing number of local studies of individual settlements and their founders. Despite several organizational weaknesses—especially the absence of a concluding chapter—Benigno's study of Paceco exemplifies the exceptionally high quality of much of the local history of early modern Italy. The strength of this historical tradition derives not only from intimate familiarity with small-scale topography and geography but also from a willingness to pose larger questions and to root "local knowledge" firmly within the broader context of early modern economic and social history.

JAMES S. AMELANG
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GIAN PAOLO BRIZZI, editor. *Il catechismo e la grammatica*. Volume 1, *Istruzione e controllo sociale nell'area emiliana e romagnola nel '700*. (Cultura e vita civile nel Settecento.) Bologna: Il Molino. 1985. Pp. 436. L. 35,000.

The title for these six essays, edited by Gian Paolo Brizzi, is poorly chosen. They have little to do with catechisms per se, and more with the religious ambiance of the area under consideration. Loosely binding the contributions are two themes: the all-pervailing influence of Jesuit schools, even after the suppression of the society, and the general late eighteenth-century European tendency to combine moral and doctrinal instruction in the Christian faith with state insistence that pupils not only have sound doctrine but also *civiltà*, defined as preparation for living in a peaceful, productive relationship with other princely subjects.

P. Lucchi examines various educational theorists and their ideas about the purpose of education, which was not to turn out masses of people capable of reading, writing, and calculation but to effect a compromise between the Latin and vernacular schools. From D. Marchesini's essay one is inclined to conclude, despite his torturous but impressive accumulation of percentiles on calligraphy and literacy, that the results are fairly obvious even without statistical data. For example, literacy was more widespread in urban areas than in the countryside, and males had a higher literacy rate than females. The last four essays reiterate common themes: the stereotypical virtues of the *buon maestro*, religious or lay, and his unquestioned (mostly) authority over his pupils (P. Severini); the methods, both old and new, for teaching Latin and Italian grammar (R. Ballerini); the, again stereotypical, ways of teaching both manners and morals (C. Pancera). Since there were formulas for teaching and encouraging emulation, some fixed as early as the sixteenth century (principally by the *Ratio Studiorum*) and heavy borrowing by other religious orders and in turn by civic or lay schools, that educational objectives bore strong family resemblances is no surprise. The teacher was expected to be a model of devotion, modesty, self-discipline, and all the Christian virtues. If he were not and this could be proved, his dismissal was perfunctory. Surprisingly, these essays detect little French or Jansenist influence (other than the texts of Port-Royal). The Italians were evidently working from a native tradition.

The last contribution (M. Turrini and A. Valenti), despite its dual authorship, is perhaps the best in the collection. It shows plainly that habit, elaborately contrived ritual (chiefly centering about Marian sodalities), regular confession, and eucharistic reception (generally once a month) were as significant in habituating pupils to act as Christians as were formal catechisms. In other words Italy was in transition between an oral and a written culture with oral tradition still prevalent.

In the introduction Brizzi promises a second volume to deal with the influence of enlightened monarchs on education in Emelia and Romagnola. Considering the complicated political structure of eighteenth-century Italy and the difficulties this presents to historians, this collection is a brave attempt to put that history on a statistical basis.

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CARLO ZAGHI. *Potere chiesa e società: Studi e ricerche sull'Italia giacobina e napoleonica*. Naples: Istituto Universitario Orientale. 1984. Pp. xxviii, 754.

In October 1984 Carlo Zaghi signed the preface to this volume on Italy in the Jacobin and Napoleonic periods. These seven hundred pages represented the best of a scholarly output that spanned nearly sixty years and encompassed not only the history of pre-Risorgimento Italy but other topics as well, among them the history of Italian colonization in Africa.

Zaghi's lifelong love affair with archival research began when he was not quite twenty. A stringer for and later editor of *Gazzetta ferrarese*, he became intrigued with the history of the Este dynasty and its neighbors and rivals, the dukes of Mantua and the Venetian Republic. Such was the thrill of the hunt in the Ferrara archives that Zaghi continued to do research and gradually moved to the period of papal domination of the region and finally to the *triennio* (1796–99). Like other historians of his generation and training, he began with a study of politics, especially of the movement for constitutional government in the preunification Italian states, and produced biographical profiles of major Jacobin leaders. After the war, however, he shared in the revival of scholarly interest in social and cultural history. In his beloved archives he searched for clues about the social structure of late eighteenth-century Italy, the position and influence of the clergy and the nobility, the role of Francophile intellectuals, and the attitudes of the European powers toward the Jacobin republics.

A massive anthology of chapters, essays, and sketches published over many years reflects the slow maturation of the author and the ever-increasing breadth of his interests and knowledge. The volume is organized chronologically to cover the period from the 1780s to the founding of the Napoleonic Kingdom of Italy. Within this chronological framework the material is arranged by topic. A bibliography of Zaghi's scholarly and journalistic writings, compiled by his former student Pasquale Sarli, completes the work.

The book confirms Zaghi's reputation as one of the most prolific scholars of the Jacobin and Napoleonic periods. More than that, this volume highlights the pioneering nature of Zaghi's work, on which scholars in Italy and other countries have built. Thus, for instance, his analysis of social structure in northern Italy before the French revolution (chapter 1) may appear sketchy, even naive, compared with more recent works in the field. It lacks the methodological and theoretical sophistication of social history as practiced by the heirs to the Annales school or, in the English-speaking world, by Peter Laslett and Lawrence Stone. But Zaghi's insights are the fruit of decades of painstaking reconstruction of available historical sources that he opened up for others, making possible both descriptive and interpretive work. This volume is not only a monument to the industry and learning of one man but also a very important contribution to recent Italian historiography. Zaghi's own evolution from enthusiastic but not very discriminating pursuer of archival "finds" to careful student of the broad social, cultural, and political trends of the late eighteenth century exemplifies the impressive intellectual development of historical scholarship in his country, especially in the postwar period. For that reason, even readers who are not particularly interested in the events and people of the *triennio* will find this volume useful, and they will be reassured, I believe, that historical scholarship is alive and well and (sometimes) fun to read.

CLARA M. LOVETT
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CARLO ZAGHI. *L'Italia di Napoleone dalla Cisalpina al Regno*. (Storia d'Italia, number 18, part 1.) Turin: Unione Tipografico-Editrice Torinese. 1986. Pp. xiv, 813.

Since the nineteenth century, when Italy finally achieved national unity, its historians have debated the impact of French revolutionary ideas and of Napoleon's rule in the peninsula on the formation of the nationalism that led to Risorgimento. Did French hegemony from 1796 to 1814 represent the starting point of Italy's national awakening, or had it merely substituted one foreign domination for another? Was not a burgeoning self-awareness already emerging during the eighteenth century? The debate reached frenzied heights under Fascist rule. French influence was denounced as another foreign intrusion in Italy's historical evolution as a nation, and the autochthonous inspiration for Italian nationalism was found in all of Italian history before 1789. A more balanced view has been emerging in the historiography of the post–World War II period.

After more than fifty years of research and writing on the Napoleonic period in Italy, Carlo Zaghi has contributed a magisterial study of its evolution in northern Italy from 1796 to 1814. A perceptive and detailed study of Napoleon, his policies, goals, and character, the book argues that Italians first acquired a sense of national identification under Napoleon, whether they worked for him or conspired against him. To be sure, Napoleon was a despot. He crushed freedom, despoiled Italy's artistic patrimony, and subordinated Italian interests to those of a greater France. Yet, "unwittingly, more than any other man . . . [Napoleon] opened the way

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for the Risorgimento" with the changes his regime effected in the social structure of the country. By awakening forces long dormant he "taught Italians to look beyond narrow municipal confines" and to become more aware of Italy as a whole and of Europe (p. 678).

Although France refashioned political, social, and economic institutions throughout continental Italy, its influence weighed more heavily in the northern half of the country, where a new unity was imposed. By French fiat and with dizzying rapidity, the Cispadane republic of 1796 gave way to the Cisalpine in 1797. Terminated in 1799 by the retreat of the French from Italy and the return of Austria with the support of its Russian allies, it reemerged as the second Cisalpine republic after Napoleon's triumphal reentry into Milan on 17 June 1800. Two years later it was replaced by the Italian Republic and finally by the Kingdom of Italy, which endured until Napoleon's final defeat. With these changes in political designations came fundamental and radical reforms. Not only were the temporal power of the church abolished and the basis of a secular society laid but the person and authority of the pope himself were challenged. Traditional boundaries disappeared, as territories were shifted around to serve French interests. Historical differences and rivalries were temporarily forgotten. The Napoleonic Codes and uniform weights and measures and administrative procedures replaced the ancient hodgepodge. An army fighting under its own officers and flag provided Italians with a military identity.

According to Zaghi, these changes brought forth "a new generation of Italians . . . free of regional, political, social, cultural, and racial prejudices, more alive and aware than preceding and even later generations" (p. 679). For the first time in their history Italians shared a sense of their own identity and developed common national bonds. Even those Italians who opposed Napoleon did so in the name of Italian freedom and independence, rather than in defense of local privileges.

In tracing the establishment and governance of the Kingdom of Italy under the regency of Eugène de Beauharnais, Zaghi has also analyzed the impact of Napoleon's rule on the entire peninsula. Although the history of southern Italy for the same years will undoubtedly constitute a separate volume in this new UTET history of Italy, under the editorship of Giuseppe Galasso, the principles and goals that inspired Napoleon's policies in Italy and their consequences for its people are clearly set forth by Zaghi.

The book is organized in twelve chapters, plus an introduction and a conclusion. A magnificent critical bibliography of primary sources and secondary studies (pp. 683–791) contributes to the scholarly importance of the volume. Reflecting the new trends in historical research and writing, Zaghi discusses not only political and military history but also social and economic changes, which emerge as the most important results of these years. Enriched by numerous illustrations and an index of names, this volume is an example of the best contemporary Italian historical scholarship, both in content and format. If a cavil may be allowed, maps showing the shifting territorial boundaries imposed by Napoleon on his Italian domains would have been helpful, but this is a minor shortcoming in a work so rich in scholarly insights.

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ISTITUTO PER LA SCIENZA DELL'AMMINISTRAZIONE PUBBLICA. *L'amministrazione nella storia moderna*. In two volumes. (Archivio, new series, number 3.) Milan: Giuffrè. 1985. Pp. xxxvii, 1088; xi, 2364. L. 160,000.

In his introduction to one of the sections in this collection of articles Piero Aimo concedes the inherent difficulty of history as anthology: at best the individual analyses can only in part correspond to the methodological objectives "in wide-ranging cultural undertakings of this type" (p. 553). Indeed, some thirty specialists in Italian history cannot be expected to produce a coherent interpretation of how administrative institutions in the varied regions of Italy took shape, from the age of Maria Theresa to that of Giolitti. Instead, they give us specialized reports based on their own archival research. English summaries of the articles in part 1, "Administration in Modern Italy"—the vast bulk of the book—are provided, but these amount to little more than abstracts of the long and densely packed arguments that will make heavy demands on the reader's time and concentration.

Although lacking a systematic account of its subject, the study possesses a consistent theme: politics begins in rhetoric and ends in bureaucracy. Section 1 of the first part, consisting of seven essays, reinforces the argument of Franco Venturi in *Settecento riformatore*, concerning the centrality of Enlightenment idealism in modern Italian political culture. Politically subjugated and economically stagnant, the Italians lagged far behind their powerful neighbors to the north in the development of modern administrative institutions. The Old Regime did not concern itself with establishing channels of official power leading from the government to every sector of society—especially the economy, public health and hygiene, education, postal service, water rights, and conservation—to nearly the same extent that the states of the future would.

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The main strength of the first section lies in the examples—in Genoa, Bergamo, Venice, Milan, and Pavia—the authors have provided of the peninsula's backwardness and of its halting, imitative attempts to reform. Inspiration to move forward initially came from the Austrians, whose presence in Milan made that city the focal point of Italian administrative modernization, although even there the reform movement was neither homogeneous nor linear. The *illuministi* condemned what they viewed as the outmoded institutions of the Old Regime, by then overgrown with class, professional, and territorial entitlements. They intended to rationalize the exercise of administrative power in the central government by systematically eliminating competing units of authority.

Nowhere in Italy were the enlightened ideals of the reformers fully realized, and only the intervention of Napoleon ensured Italian administrative modernization. In effect, Napoleon appeared as the long-heralded prince of the Enlightenment, and his model of administrative monarchy originated in the ambitious but only partly realized political theories of enlightened absolutism. He envisaged the administrative apparatus as the means for enlightened regeneration. Accordingly, the Napoleonic state set hitherto unapproached standards for bureaucratic intrusion in the lives of ordinary citizens.

The second section of part 1 contains six accounts of Italy's first major experience with modern state administration. As in the first section, the overall design of these chapters is not systematic; each one stands on its own, yet the cumulative impression is uniform. Statistical surveys began to pile up as the government sought to learn more about society, the better to change it in accordance with bureaucratic agenda, tax it more profitably, and conscript soldiers more efficiently. The Kingdom of Italy received a statistical office, a gift from the mother bureaucracy in Paris. Not surprisingly, its surveys helped spread the belief, essential to the growth of bureaucracy, that without the information provided by their indefatigable authors the vitality of government and of civilization itself would be sapped. The movement of governmental paper possessed a recognizably modern shape in the Napoleonic empire. One of the best essays in the section, on Gian Domenico Romagnosi, meticulously depicts how the author of *Principi fondamentali del diritto amministrativo* (1814) celebrated the achievements of Napoleon's bureaucratic revolution and why this celebration helped establish the sanctity of authoritarian paternalism in the country's study and practice of public administration.

Obsessed by the vision of a monolithic empire, Napoleon ignored regional differences in Italy. Regulations for Milan were also imposed on Naples, with dismaying results. The Italian nation-state repeated this mistake later in the century, and the thousand-page third section of part 1 considers the difficult post-Risorgimento period during which a large part of the population viewed the national government as an oppressive power. The administrative reforms of Francesco Crispi in 1888 further centralized the Italian government, until then based on the original French-Piedmontese administrative model of 1859. More than ever before Rome viewed local administrations as only the agents of its will. Vittorio Emanuele Orlando lent his enormous prestige as a scholar to the campaign for "vigorous centralization," on the grounds that Italy lacked the political class necessary for self-government. Throughout the period from 1890 to 1915 state jurisdiction grew at the expense of communal government.

Local governments received a vital new impulse from the movement known as *municipalizzazione*. They rapidly took over such city services as communications, transportation, gas, electricity, education, and public health. Deprived of political power by the centralizing process, local administrators nevertheless greatly assisted the bureaucratization of the Italian economy as first the practice and then the theory of classical liberalism fell into disuse. Under the pressure of breakneck urbanization, municipal functions, too, had to be rationalized, and this meant a sharp curtailment of unrestricted business practices in sectors of the economy deemed essential to the public good. Thus, the "social vocation" of the commune became legitimized under the auspices of the state's higher authority, and a law of 1903 rendered this relationship fully explicit.

What Camillo di Cavour had begun and Crispi had augmented Giovanni Giolitti completed: the erection of a tightly centralized state administration. The thirteen essays in section 3 provide a wealth of detail about that lengthy and, ultimately, unsuccessful development. Fascism undid the work of Italy's liberal government. With only a few exceptions, notably the essays on the chambers of commerce and the National Rice Corporation, the authors do not take us beyond the Giolittian period. The glimpses we get of the Fascists show them to have been protagonists of radical centralization. However, far from carrying forward the administrative revolution of the liberal state, they first perverted it and then obliterated it by creating a government so different in degree as to be different in kind. This is obviously not material for the general reader, who in section 3 would find excessive detail about the rivalries and office politics in various government ministries, but here the specialist in modern Italian history will find invaluable statistical and biographical information about the personalities, ideas, and practices that shaped the country's administrative structure.

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The book closes with a two-hundred-page part 2, made up of three essays on the history of administration in France, Germany and Austria, and Spain. These are much more general in scope than the articles in part 1, but the contrast they afford highlights the implicit thesis of the book, that the history of Italy's political administration took the course it did as a result of the country's sequential subservience to Austria, France, and Piedmont.

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LUIGI DE MATTEO. Holdings *e sviluppo industriale nel Mezzogiorno: Il caso della Società Industriale Partenopea, 1833–1879*. (Ricerche di storia economica, number 3.) Naples: Istituto Italiano per gli Studi Filosofici. 1984. Pp. 138.

In 1833 and 1834 the protectionist policies of the new monarch, Ferdinand II, stimulated an investment boom in the Kingdom of Naples. The number of joint-stock companies doubled in Naples. These new finanacial companies were interested in speculative, short-term investments that yielded high returns. They contributed, however, to the revival of the sugar, textile, glass, and leather industries in Naples.

Luigi De Matteo has studied the history of one of the most successful of these companies, the Società Industriale Partenopea—from its birth in 1833 to its demise in 1879. In its early years the Partenopea was a financial holding company involved in discounting and lending operations, in credit activities, and in the dye-stuff and printing materials industry. In 1834 it extended its activities to the manufacture and sale of woolen carpets, glue, silk textiles, sugar, and porcelain. In 1837, after an abortive attempt to establish a large-scale plant to produce cotton yarn with John Cockerill, a Belgian industrialist, it built a mechanical spinning mill at Sarno for the manufacture of linen yarn and textiles. The decision to produce linen rather than cotton largely resulted from competition in Salerno from the Swiss cotton firms that had set up in southern Italy to avoid the effects of the Bourbon tariff.

The Partenopea flourished through the 1840s and 1850s, becoming the largest manufacturer of linen textiles in southern Italy. Under an able and efficient manager, Augusto Sideri, it yielded a respectable return to its stockholders. The industry, however, relied heavily on tariff protection and depended exclusively on the internal market for consumers. Because of its low level of technology and limited diversification of products, it was unable to compete with English and French linens in the international market.

In the decades after unification, particularly with the recovery of cheap cotton textiles after the U.S. Civil War, the linen industry encountered difficulty. The Partenopea was unable to respond to the challenge. Faced with heavy indebtedness, needing fresh infusions of capital, lacking proper entrepreneurial guidance, and confronted with new free-trade treaties, falling prices, and growing competition from more efficient producers (in northern Italy and Europe), the Partenopea collapsed. Parenthetically, at this time Swiss cotton manufacturers (Schlaefer, Wenner, Vonwiller, and so on) in Salerno (the Manchester of the south) had begun a highly successful reorganization of the cotton industry. The mentality of the Swiss industrialists was quite different from that of the Italian entrepreneurs of the time.

Agriculture, however, not commerce, was the main business of southern Italy in the nineteenth century. Investment was attracted to more traditional branches of economic activity, and wealth and power depended on monopolistic privilege conferred by the state. Economic power rested not on trade but on capital and credit—particularly on credit to the state. This has been well documented in John Davis's recent work, *Merchants, Monopolists, and Contractors in Bourbon Naples* (1981).

De Matteo nevertheless has skillfully analyzed the financial, economic, and administrative activities of the Partenopea. Not the least of his contributions has been to relate the complex interconnections of his story to the broader historical framework.

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FILIPPO SPATAFORA. *Il comitato d'azione di Roma dal 1862 al 1867: Memorie*. Volume 2. Edited by ANNA MARIA ISASTIA. (Domus Mazziniana, Pisa: Collana Scientifica, number 17.) Pisa: Nistri-Lischi. 1984. Pp. 628–1224.

The pious tradition of publishing the correspondence of even minor patriots is one of the assets of research on the Risorgimento, and Filippo Spatafora's memoirs are in that tradition. Assembled a century ago as an old man's effort to set the record straight, more than half of the text consists of the letters he sent and received a generation earlier. Spatafora was a central figure in the Roman comitato d'azione, and his memoirs and correspondence provide a detailed account of the committee's efforts during the 1860s to build within papal Rome a revolutionary base that would both complete Italian unity and transform the moderate Italian kingdom into a more Mazzinian polity. The first volume covered 1862–63, and this one deals (unevenly) with

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the next four years in more than five hundred pages of fine print.

Much of the correspondence was with Giuseppe Mazzini and the Tuscan radical, Giuseppe Dolfi; as these names suggest, those associated with the committee were more or less republican. Despite a certain vagueness and many disagreements, they shared a Mazzinian dream of Italy's regeneration (even when keeping their distance from Mazzini) and were fervent democrats. Relentlessly serious, they explored every event to determine the stand they should take, worried over the comments of every newspaper, and wrote wordy appeals to the Roman people alternately urging them to boycott festivities in honor of Pius IX and to undertake some "virile action."

Theirs were the busy and self-important lives of conspirators, signing their *noms de guerres* to coded messages that were not always delivered or fully understood and furiously analyzing every move of their competitors in the moderate comitato nazionale. This was serious business; many of their number were imprisoned, and some died in jail. Spatafora provides statements of the (pathetically small) sums that they received from abroad and meticulous accounts of how that money was spent to aid prisoners, buy arms, and print posters. Awash in intrigue, Spatafora was at home with the involute politics of the papacy. He had sources in the bureaucracy with whom he could discuss in detail the corruption of Cardinal Antonelli and the latest news about the pope's health. Speculation about the import of the government's most minor moves was taken as evidence of a well-informed party ready for any event. Like the preoccupation with Pius IX's imminent death, it instead reveals bankruptcy.

These figures who had once imagined Rome at the center of world history now felt that the pressures from outside—the policies of the Italian government, the power of Napoleon III, the influence of diplomacy, or Roman moderates in exile—could only frustrate their cause. Too deeply engaged to abandon their efforts, they did not have a sense of history that could give them much hope. They candidly noted that the moderates were gaining and that the republicanism of some of their own followers was suspect, but they could only ascribe these developments to individual weakness and sinister plots. By 1866 they were lamenting the growing influence of Mikhail Bakunin and his "nordic theories" in Naples. These moral men wrote with power about the corrupt incompetence of papal bureaucracy and the social insensitivity of the moderates. They proudly insisted that the Roman people were "neither corrupt nor superstitious" but merely lacked political education. So did the men of the comitato d'azione. Interesting for what they say and for the minor biographies they provide, these documents are most revealing in their silences. Spatafora says nothing about mobilizing mass support or postrevolutionary political programs. Even when he assembled these memoirs, he was primarily concerned to show his personal rectitude.

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DAVID FORGACS, editor. *Rethinking Italian Fascism: Capitalism, Populism, and Culture*. London: Lawrence and Wishart; distributed by Humanities, Atlantic Highlands, N.J. 1986. Pp. xxi, 209. $29.95.

This book attempts a revision of current thinking on Italian Fascism that goes beyond historical questions to include a consideration of such cultural problems as cinema and literature. Seven of the essays were originally presented as public lectures in Great Britain in 1984, and two others, plus the editor's introduction, were written especially for this collection. The lectures were presented for a nonspecialist audience and retain this accessible character in print.

The volume clearly represents the position of leftist British academics or leftist Italian academics who hold positions in Great Britain on Italian Fascism. Consequently, the canonical views of Antonio Gramsci or of Nicos Poulantzas (a Greek Marxist theorist and author of *Fascisme et Dictature* in 1968) seem to inform many of the most overtly ideological essays in the volume. As is so often the case, the more concrete and specific essays are the most useful, whereas those that attempt in only a few pages to present a reevaluation of Marxist theory on Italian fascism will leave the reader less satisfied. Geoffrey Nowell-Smith's "The Italian Cinema under Fascism" and Lino Pertile's "Fascism and Literature," for instance, are first-rate surveys that avoid polemical positions and would be rewarding reading for both students and scholars. Tobias Abse's "The Rise of Fascism in an Industrial City: The Case of Livorno, 1918–1922" and Jonathan Steinberg's "Fascism in the Italian South: The Case of Calabria" are equally interesting case studies based on extensive archival research; they juxtapose two diametrically different experiences and underline the difficulty of making generalizations about Italian Fascism. Paul Corner's "Liberalism, Pre-Fascism, Fascism" is another useful consideration of the transition between Italian liberalism and Fascism. Other essays analyze Marxist thinking on Fascism (David Forgacs), Fascist policy on women and the family (Lesley Caldwell), oral memory of the Fascist period among working-class Italians of Turin (Luisa Passerini), and the revisionist positions taken in the anthology itself (Roderick Kedward).

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Although the editor obviously hopes this book will be adopted in classrooms, and a need for a book that could be used by a number of disciplines seems obvious, this volume should be given to American undergraduates with some caution. In the essays I have noted for their special interest, the obvious ideological slant typical of some of the contributions is absent. At least one of the motivations for this revision of perspectives on Mussolini's Italy is the expressed desire of some of the contributors to apply a revised definition of "Fascism" to liberal regimes that are not to their liking (especially Margaret Thatcher's government). In this sense, this collection inadvertently tells an American reader something interesting about the political views of British academics.

In addition to the essays, the volume contains some very useful material for the student: a chronology of the major events in Italian history during the *ventennio*, a handy list of abbreviations associated with the Fascist regime, and a glossary of Italian terms employed in discussions of Fascism.

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ŽIVAN MILISAVAC. *Istorija Matice srpske*. Volume 1, *Vreme nacionalnog budjenja i kulturnog preporoda 1826–1864* [History of the Serbian Matica. Volume 1, The Period of National Awakening and Cultural Rebirth, 1826–64]. Novi Sad: Matica Srpska. 1986. Pp. 974.

The oldest continuously operating literary and cultural society in Southeastern Europe is Matica srpska, a title that can only awkwardly be rendered into English as, perhaps, the Serbian Matrix. Živan Milisavac's nine-hundred-page monograph, which is the product of a lifetime of research and writing, describes the first forty years of the society in mind-numbing and fascinating detail—mind-numbing because he mentions every person who applied for a scholarship, submitted an article, or attended a meeting of the society but fascinating because his patience and love of detail provide unique descriptions of the cultural and social history of Serbs in the Austrian empire during these transitional years.

Matica srpska was founded in Pest by seven middle-ranking Serbian merchants to support the publication of a new journal introduced two years earlier in Novi Sad. But during the period of its ensuing publication in Pest, neither this journal, *Letopis*, nor the series of monographs that was the society's second major cultural endeavor attained the influence or quality their founders hoped for. One reason was the underdeveloped state of Serbian literature, but another was that Matica ignored the handful of authors who were producing high-quality work. Concentrating on a series of futile literary and grammatical competitions, it lost the support of the most important scholars of the day, such as Pavel Šafárik, and its middle-of-the-road position on issues of language and orthography prevented cooperation with Serbia's most able linguist, Vuk Stefanović Karadžić.

The most important aspect of the early history of the society was not literary at all, but the participation of Sava Tekelija, a wealthy Serbian nobleman who, because he had no family except an estranged wife, took an interest in the society in his old age and eventually bequeathed it his considerable fortune. Tekelija's money, along with sizeable gifts from others, made Matica srpska and its associated foundations the richest private foundation in the Habsburg empire. In the long run Tekelija's bequest assured the future of the society, but in the short run it undermined its purpose. Meetings of the society became devoted to the minutiae of financial management, which the mercantile and legally trained membership understood and appreciated, and Matica turned into more a lending institution than a cultural one. The increased respectability of the organization attracted conservative members who were unwilling to permit editorial changes that might improve the dismal performance of *Letopis* (its circulation never reached 250). The society also became bogged down in an interminable series of litigations with Tekelija's estranged wife. When the changed political situation in Hungary at the end of the Bach regime permitted the emergence of Novi Sad as the cultural and political center of Serbs in Hungary, the increasingly isolated leadership in Pest finally agreed, albeit in a laconic and disorganized way, to move the society to Novi Sad, where it has prospered ever since.

Milisavac's description of the lawsuits over Tekelija's fortune well illustrate the social constraints of the Habsburg empire, as Matica struggled to find sufficiently distinguished individuals to back its cause. The description of the institution Tekelija founded to aid impoverished Serbian students (the Tekelijanum) reminds us of the serious problems of health and sanitation common to the era. Of 105 needy students who received these scholarships, 12 died during the term of their stipend. By the late 1850s the health of those living in the Tekelijanum was so bad that a doctor was visiting the institution every day. Milisavac's mention of the educational and social background of most of the people he names not only shows that legal training was the main route to social mobility for Serbs by the 1840s but also demonstrates that before 1848 the Habsburg empire contained a number of rich and even noble Serbs, most of whom, however, were thoroughly Magyarized.

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In short, this massive book is both a compendium of data and the definitive work on its rather narrow subject. At a time when the historical profession is being swept by theoretical winds of great variety, it is not unsatisfying to settle down for a long read with an old-fashioned magnum opus.

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JAN KOZIK. *The Ukrainian National Movement in Galicia, 1815–1849*. Translated by LAWRENCE D. ORTON and ANDREW GORSKI. Edited by LAWRENCE D. ORTON. Edmonton: Canadian Institute of Ukrainian Studies, University of Alberta. 1986. Pp. xx, 488.

The development of Ukrainian national consciousness in Galicia in the two decades before the revolution of 1848 and the attempt to translate ideals into political institutions during the springtime of peoples follow a path familiar to historians of other national movements in the Habsburg Monarchy. Jan Kozik treats the Ukrainian pattern from within and consequently emphasizes the writings of its participants as the primary sources. The result is an immensely detailed work, which, although it brings out the unique characteristics of the Ukrainian movement, never loses sight of its broader, European connections.

In a sense, Kozik's work is a collective portrait of the Ukrainian generation of 1848 as the bearers of Ukrainian national consciousness. As elsewhere in Eastern Europe, they were intellectuals, and, because Ukrainian society was overwhelmingly rural, they were also clergymen. By training and temperament they put great store by education—"enlightenment," they called it—and were certain that if ignorance could be overcome beneficial social change would follow in due course. The building of schools and the writing of textbooks were for them civic duties, but questions of higher culture aroused their enthusiasm. The work of the so-called Ruthenian Triad—Markiian Shashkevych, Ivan Vahylevych, and Iakiv Holovatsky—who led the national movement with their studies of the Ukrainian language and history in Galicia and their attempts to found a modern literature, are reminiscent of the activities of Slovak and Romanian intellectuals of Hungary at a somewhat earlier period.

The controversy over the literary language and the alphabet, in which the triad was deeply involved, was a typical intellectual preoccupation of the time. Kozik keeps the many strands of this complicated issue from becoming entangled and judiciously uses the resulting data to measure the level of Ukrainian national consciousness. Strikingly, in the 1830s the polemics were carried on mainly in Polish, evidence of the enormous influence of Polish literature and language among Ukrainian intellectuals and the relatively limited development of Ukrainian. Yet, just as important in the 1840s was the anti-Polish current, which intensified under the impress of the romantic notion that language was the clearest expression of nationality and that its loss signaled the disintegration of the nation itself. The concomitant debate over which alphabet, Latin or Cyrillic, was proper for the new literary language went beyond linguistic problems. Those who objected to the Latin alphabet argued that its use would strengthen ties with the West and divide the Ukrainians of Galicia from their conationals under Russian rule and other Slavs. Consistently hostile to the West, they held its institutions, notably feudalism, responsible for the disruption and disintegration of medieval Slavdom.

Kozik treats 1848 as a year of crisis for the Ukrainian national movement, as one of difficult choices and rapid political maturing. He describes at length the programs and the organizations of Ukrainian intellectuals, the divisions within their ranks between conservatives and liberals, and the role of the clergy and peasants. But he makes clear that events in Galicia were dominated by the insurmountable rivalry between Ukrainians and Poles. Their unwillingness to compromise as they strove to achieve national ambitions led to one of the characteristic tragedies of 1848 in Eastern Europe. Ukrainians and Poles who shared a common concern for civil and political liberties found themselves at odds over national rights. Liberal (and conservative) Poles offered the Ukrainians equality, but only as individuals in a Polish Galicia or in a reconstituted independent Poland. Liberal (and conservative) Ukrainians, who were anxious to preserve their own language and nationality, could not accept these terms. The beneficiary of this conflict was antiliberal and antinational Austria. The story is strikingly reminiscent of the struggles between the Magyars and the Slavs and Romanians in Hungary.

This book, a translation of two monographs that appeared in Polish in 1973 and 1975, is a singular contribution to the scholarly literature on Ukrainian nationalism in English. It deserves to be read also by those interested in the comparative study of modern national consciousness.

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MICHAEL WORBS. *Nervenkunst: Literatur und Psychoanalyse im Wien der Jahrhundertwende*. Frankfurt a/M.: Europäische Verlagsanstalt. 1983. Pp. 383. DM 48.

JACQUES LE RIDER. *Le cas Otto Weininger: Racines de l'antiféminisme et de l'antisémitisme*. (Perspectives critiques.) Paris: Presses Universitaires de France. 1982. Pp. 255. 100 fr.

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Vienna has justly come to be regarded as a historic location for thinking and writing about sexuality, arguably the central theme of Viennese modernism in the generation or two after 1890. The books under review explore this thinking about sexuality from the perspectives of two psychologists who became famous in Vienna during the first decade of the century: Sigmund Freud and Otto Weininger. Both books draw on recent work by American historians (particularly Carl Schorske and William McGrath) to develop arguments about the context of Viennese culture and politics that go beyond conventional formulations about eroticism and morbidity, but they also emphasize the wider European context of psychiatry and *belles lettres*. Michael Worbs explores the connections between psychoanalysis and Austrian literature, and Jacques Le Rider recalls that much of this thinking about sexuality amounted to men thinking about women.

Nervenkunst: Literatur und Psychoanalyse im Wien der Jahrhundertwende is an informative literary-historical study, based on Worbs's dissertation. Worbs is primarily interested in the reception of Freud's thought by *Jung Wien*, and his main sections describe Freud's relationship to Karl Kraus, Arthur Schnitzler, and Hugo von Hofmannsthal. Worbs wants to show that Freud and *Jung Wien* (to which Schnitzler and Hofmannsthal, but not Kraus, belonged) shared common intellectual and cultural ground. Although Freud had only minor personal and intellectual connections with literary Vienna, Worbs argues that the concerns of nineteenth-century psychiatry and Austrian literature converged in an introspective *Nervenkunst*; both psychoanalysis and *Jung Wien* developed psychological, inward visions out of the naturalism and positivism of the late nineteenth century.

Much of the information will be familiar to those who work in the field, but Worbs's thorough research offers interesting juxtapositions, especially regarding Schnitzler and Hofmannsthal. Worbs argues that the differences between Freud and Kraus have been exaggerated because of the personal role of Fritz Wittels in mediating the exchange of ideas between the two men. Certainly, Freud and Kraus were both committed to sexual enlightenment, but they differed in their views on the relationship between sexuality and creativity. The real similarities, however, are between Schnitzler and Freud. Freud saw Schnitzler as his double, and Schnitzler was far more interested than the other poets of *Jung Wien* in psychoanalysis. Like Freud, Schnitzler emphasized the power of sexuality and the yearning for union that is often a threat to social convention and to the security of the ego. But Schnitzler's notes after 1921 show that he considered Freud too speculative and systematic, and the actual influence of Freud's thought on his work was slight. Hofmannsthal shared Freud's impulse to psychologize classical myths, and Worbs emphasizes the similarity in their reception of the ancients. But Freud and Hofmannsthal lived in entirely different social and intellectual worlds, and Hofmannsthal regarded Freud as "an absolute mediocrity" (p. 267). Freud, for his part, had the wisdom not to expect dramatists to portray his theories on the stage; he agreed with Robert Musil that, rightly understood, there is no such thing as a psychological literature. The yield of these juxtapositions is modest, and Worbs does not provide his long book with a conclusion. But his central argument seems to be that Freud's work inadvertently matched the mood of *fin-de-siècle* literature; Freud liked naturalism and avoided reading the moderns, but he helped overcome the naturalism he admired.

Otto Weininger (1880–1903) had no use for *Jung Wien*, and Freud saw little promise in the young man's dissertation. Weininger was close to Kraus in his use of the theme of sexuality to condemn the culture of liberal Vienna, and he was perhaps the most radical critic of conventional notions of sexuality and gender at the turn of the century. Le Rider argues that Weininger was actually more famous than Freud in the first decade of the century, and feminists will be surprised to learn that Weininger regarded psychoanalysis (like science and capitalism) as a feminization. *Le Cas Otto Weininger: Racines de l'antiféminisme et de l'antisémitisme* reflects recent French interest in Austrian history and literature and the wider European revival of interest in Weininger in particular; it is now the best book on Weininger, and Le Rider effectively sets Weininger in the intellectual context of his time.

As Le Rider emphasizes, Weininger formulated his critique of culture as an attack on femininity and the Jew (he was himself Jewish but converted to Protestantism). Le Rider is kinder to Weininger than most critics, but he is still reluctant to take seriously Weininger's distinction between the feminine principle (*das Weib*) and individual women (*die Frau*), and Le Rider cannot always resist the temptation to equate antifemininity with antifeminism. But to see Weininger's critique of sexuality (which he regarded as feminine) simply as conventional male opposition to feminism or women is not entirely fair. At the very least Weininger belongs (like Schopenhauer and Nietzsche) to the honorable misogynists of a profoundly sexist high culture. But his radical argument on behalf of human freedom is almost breathtaking in its opposition to conventional social roles and the traditional manipulation of women as sexual objects and mothers. Weininger preferred to see the human race die out (here he meant to be more consistent than St. Augustine) than to postpone human freedom for women. He suffered so terribly under these ideas that he took

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his own life at the age of twenty-three, only a few months after publishing *Geschlecht und Charakter*. Le Rider faults him for confusing literature and reality and emphasizes the pathos of this earnest young man, but Weininger also deserves respect for demanding an immediate end to human slavery.

Le Rider emphasizes the links between Weininger's misogyny and his anti-Semitism and the connection of this way of thinking to political reaction. But he also links Weininger's ideas to nineteenth-century philosophy, literature, and science, and he argues that Weininger should not be regarded as marginal or idiosyncratic, given the strong (and often sympathetic) responses of his contemporaries. Perhaps the most interesting aspect of Le Rider's account is his presentation of Weininger in the context of modernist doubts about romantic union and sexuality. Le Rider argues that Weininger was "one of the first to emphasize the great theme of modernity: existential solitude" (p. 239). It is not entirely clear, to be sure, that modern existential isolation has liberated itself from the romantic dialectic of yearning and despair, since modernist isolation may be only a waste product of the original romanticism.

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JOHN D. BELL. *The Bulgarian Communist Party from Blagoev to Zhivkov*. (Histories of the Ruling Communist Parties.) Stanford, Calif.: Hoover Institution Press. 1986. Pp. xii, 202. $9.95.

This is the tenth volume to be published to date in the series, "Histories of Ruling Communist Parties," edited by Richard F. Staar. The nine earlier volumes include four on ruling Asian Communist parties (those of North Korea, Vietnam, Mongolia, and Afghanistan), four on East European Communist parties (those of Poland, Hungary, Czechoslovakia, and Romania), and one on the pivotal and hegemonial Soviet Communist party.

Bulgaria is the only so-called socialist state of Eastern Europe that has never experienced a crisis in its relations with the Soviet Union since the end of World War II. In return for its sustained loyalty and docility, Bulgaria receives substantial Soviet economic support, inflated prestige within the international Communist movement, and military protection. This is important for a small country that lost three consecutive wars in the twentieth century (the Second Balkan War of 1913 as well as both world wars) and whose territorial integrity is even now not fully acknowledged by its largest neighbor, Yugoslavia. Thus, although it may not be heroic, Bulgaria's accommodating stance toward the Soviet Union appears to be rational and is certainly prudent.

Having established his solid professional reputation in 1977 with *Peasants in Power: Alexander Stamboliski and the Bulgarian Agrarian National Union, 1899–1923*, John D. Bell has now written a book of a quite different and in some ways more difficult genre. The volume under review is a survey of a century of socialist and then Communist political life in Bulgaria (and of governmental power since 1944) condensed into 147 pages of text and 10 pages of appendixes. The distillation required for such a condensed study is surely more difficult than a comprehensive treatment that, say, 400 pages would have permitted. Inevitably, other specialists on Bulgaria or on comparative communism will regret that some of their favorite episodes, characters, or lessons have been truncated or omitted. But such omissions are inevitable in this type of survey—and students will appreciate its incisive brevity. The book is objective, thorough, reliable, and useful. Only in the last half of the last chapter, in which he approaches the present moment, does Bell occasionally (and understandably) have difficulties in maintaining sufficient distance and perspective to distinguish between solid trends and ephemeral deviations.

Three small errors should be corrected in any future reprinting of this productive book: in September 1953 Bulgaria marked the ninth (not the eighth) anniversary of the Communist seizure of power in 1944 (p. 112); in 1958 the country marked the thirty-fifth (not the fiftieth) anniversary of the Communist September Uprising of 1923 (p. 118); Todor Zhivkov has not held power longer than any other Soviet-bloc leader, being surpassed by North Korea's Kim Il Sung (and, in their days, by Yugoslavia's Tito and Albania's Enver Hoxha, although Bell might counter that their respective states had ceased to belong to the Soviet bloc in 1948 and 1968 [p. 125]).

JOSEPH ROTHSCHILD
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JANOS BERECZ. *1956: Counter-Revolution in Hungary; Words and Weapons*. Budapest: Akademiai Kiado. 1986. Pp. 223. 160 Ft.

This thin volume is an English-language reissue of a book published in Hungarian by Janos Berecz ten years ago. Its major themes are that the revolution in 1956 was really a counterrevolution, that it was fomented by imperialist machinations and aimed at restoring "feudal-capitalism," and that only the "fraternal assistance" of the USSR saved the true revolutionaries—that is, Janos Kadar and his allies in the Hungarian Communist party. The study, if one can

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call it that, was received with disdain and disgust by a large number of people on its original publication; it has not been improved by the competent English translation. Printed so that it will be available for the thirtieth anniversary of the crushed revolution, this diatribe lacks both factual exactitude and decent humility before one of the great tragedies of Hungarian history. It is full of sarcasm such as, for example, the statement, "One report on the newly established Fliers Association and the passionate determination of its members to fly in a 'free country, on free wings and in a free air' must have brought tears of pride to many of its readers" (p. 146). His assertion on the same page that irresponsible elements began the mass arrest and persecution of "people loyal to socialism" implies a widely practiced terror against loyal Communists and ignores the remarkable infrequency of acts of violence against people loyal to Communism, given the hatred with which Stalinism and the Soviet Union were viewed. His figures showing that few people were even wounded by Soviet troops—and apparently none were killed—are contradicted by both East European and Western observers who estimated that at least two thousand people were killed. The documentation serves the documentor: propaganda meets its goal of providing untruths based on false data made to be hidden in "scholarly" guise.

The author is one of the more intelligent members of the younger generation of Hungarian Communist leaders. If his goal was to receive approval of Soviet progagandists for his hardline stance on an issue that even Kadar—the party's long-time general secretary and the man in charge of Hungary's destiny since 1956—calls a national tragedy, he has succeeded admirably. If he intended to demonstrate to the West that the revolution of 1956 was the result of Western machinations (chapters 2 and 3), he failed to convince anyone. And, if the eternal loyalists of the Hungarian Academy's respected publishing house intended this volume as an academically sound treatise on the subject, they have merely succeeded in denigrating their own efforts at convincing the West that objective scholarship is alive and well in Hungary. It is a pity that an intelligent man such as Berecz, one of those truly in contention to inherit the mantle of Kadar, stooped this low.

IVAN VOLGYES
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MANFRED HILDERMEIER. *Bürgertum und Stadt in Russland 1760–1870: Rechtliche Lage und soziale Struktur.* (Beiträge zur Geschichte Osteuropas, number 16.) Cologne: Böhlau. 1986. Pp. xix, 689.

To describe the legislation that governed the urban population of the Russian empire from 1760 to 1870 would constitute a major feat. So, too, would an analysis of patterns of social and economic evolution among the merchants (*kupechestvo*) and petty townspeople (*meshchanstvo*) in this period. Manfred Hildermeier has done both in a major study that portrays the complex relationship between the tsarist state and the urban social estates (*sosloviia*).

Hildermeier's work summarizes the findings of Western and Soviet researchers and presents a wealth of statistical and descriptive material drawn from laws and archives. Part 1 weighs the impact of legislation promulgated by Catherine II and her successors, and part 2 describes the guild reform of 1824 and the tax legislation of 1863–65, with special attention to the economic activities and mores of the urban population. No less than fifty-seven tables provide data on occupation, wealth, guild membership, and urban and rural location of industrial enterprises.

The great strength of this work lies in its clear depiction of the laws by which the tsarist regime regulated the urban estates, primarily to ensure the collection of revenues, even at the expense of commercial and industrial development. Despite half-hearted efforts to create a vigorous urban elite, the tsarist government refused to allow merchants to share the economic privileges of the gentry, primarily the ownership of land populated by serfs. The resulting flight of wealthy merchants into the gentry (*Standesflucht*) only weakened the merchant estate still more.

At the base of the social pyramid in the Russian city, the situation was even worse. Petty townspeople, unable to compete against serf artisans, lived mainly as servants, itinerant laborers, gardeners, and criminals: "a social estate without a defined function and without a social identity" (p. 464). Although the number of urban inhabitants more than doubled between 1719 and 1811, their share of the total population rose only slightly, from 3.6 to 3.8 percent, and even if the populous Polish lands annexed by Catherine were included only 4.4 percent of the tsar's subjects resided in cities in 1811 (p. 162).

Despite the abundance of factual detail presented here, the book raises some questions that researchers might well consider in the future. Hildermeier devotes much space to descriptions by foreign travelers. A clearer and more nuanced view from within is needed. Granted, few merchants could both read and write (35.9 percent in Saratov in 1844, according to table fifty-seven), and Hildermeier has gleaned much from the meager memoir literature. Still, works by such writers as Vladimir P. Riabushinskii, who left a remarkable treatise on the social and religious attitudes of his Old Believer forebears,

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might well have supplemented the observations penned by foreigners.

Second, as the author admits, his portrayal of urban life represents a distillation of reality based primarily on St. Petersburg, Moscow, and the major settlements of the central region, such as Vladimir and Nizhnii Novgorod. But what of Riga, Warsaw, Odessa, and other cities that did not fit this mold? The book somehow appears too short, despite its great length, to do justice to crucial regional variations.

Finally, Hildermeier calls the laws of 1863–65 "a de facto dissolution" of distinctions between urban estates (part 2, chap. 5), and certainly the granting of membership in the merchant estate to anyone who purchased a first- or second-guild certificate constituted a major departure from the traditional notion of hereditary occupational groupings. But the estate structure—essentially a set of social categories imposed on society by the autocratic state to promote its own interests—remained the core of tsarist social policy from 1649 onward and could not be so easily demolished. In some cities strong estate loyalties among the merchants persisted well into the twentieth century. Just as serfdom is the key to understanding the Russian city in the century before 1861, so the autocracy's determination to perpetuate the estate structure constitutes the primary fact of Russian social history from 1861 to 1917. It seems, therefore, an exaggeration to assert that the Municipal Reform of 1870, the educational incentives of the Military Reform of 1874, and the proliferation of banks, railroads, and joint-stock companies led to "a thorough change, especially in the upper ranks of the merchant estate, and to the emergence of an industrial 'bourgeoisie' of the modern type" (p. 599).

These observations are not intended to detract from Hildermeier's impressive scholarly achievement. American professors need only point to this massive volume to persuade graduate students in Russian history of the necessity of knowing German.

THOMAS C. OWEN
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M. A. ALPATOV. *Russkaia istoricheskaia mysl' i zapadnaia evropa (XVII–pervaia polovina XIX v.)* [Russian Historical Thought and Western Europe from the Eighteenth to the First Half of the Nineteenth Century]. Moscow: Nauka. 1985. Pp. 269. 1r. 70 k.

At the time of his death in 1980 M. A. Alpatov had published two volumes of his extended study entitled *Russian Historical Thought and Western Europe*, one dealing with the twelfth to the seventeenth century (1973) and one focusing on the seventeenth and first quarter of the eighteenth century (1976). The current monograph, although not a completed manuscript—it literally ends in midsentence—brings the story forward approximately to the middle of the nineteenth century.

To compose a review of a volume that is in such a manifestly unfinished state is not easy. One hesitates to be overly critical of certain obvious omissions—the book lacks an introduction and conclusion, and several of its sections appear to be little more than preliminary sketches that never were written up in full—simply because one does not know what Alpatov would have done if given more time. Nevertheless, the text is complete enough to allow at least a general assessment that is on balance rather mixed.

First, let me discuss the positive features. Alpatov clearly was well read in several subjects, including Russian and French thought, and he chose to place his narrative squarely in a European context. Frenchmen and Germans, whether from abroad or in residence in Russia, are seen in discourse with Russians, and vice versa. In its broad outlines, therefore, the book stands above the overly nationalistic style of writing intellectual history that dominated the Soviet historical establishment during much of Alpatov's professional life.

The book also contains a number of intelligently argued discussions of important figures who often suffer from underexposure in Soviet scholarship. The treatment of Petr Chaadaev stands out in this regard, as do the informative and generally judicious accounts of August Schlözer's historical views, M. F. Orlov's conceptions of society and economy, and the impressions of eighteenth-century French travelers in Russia.

But these highlights are too often obscured by the books more visible shortcomings. Most glaring is the lack of any apparent focus, thesis, or overall design. The choice of topics often seems to be random, and many of the issues that are mentioned are given insufficient attention. In part this problem arises from Alpatov's troublesome format, which consists of numerous short vignettes of several authors, be they historians, polemicists, memoirists, or travelers, within a single chapter. Such a method requires the author to connect the sketches carefully by means of a narrative or an argument, and this Alpatov fails to do.

The treatment of intellectuals outside of Russia is largely unsatisfactory. Alpatov has an inflated sense of the ascendancy of French thought in the eighteenth century; English and German thinkers are dismissed in a few summary pages. Yet even his characterization of the French Enlightenment is a largely formulaic and cliché-ridden disquisition on "the age of reason."

At times Alpatov's Continental perspective deserts him altogether, and in its place he advances

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extravagant claims for the worth of Russian thinkers. M. V. Lomonosov's championing of anti-Normanism, for example, is praised as a major advance in the scholarship of early Russian history. Fair enough. But then Alpatov takes us on a journey through Lomonosov's views on ancient Greece and Rome, which he concludes with the astonishing observation that "subsequent bourgeois historiography of the nineteenth and especially the twentieth century added nothing fundamentally new to [Lomonosov's] picture of the decline of the Roman empire. And as a rule it went backward" (p. 69). Equally overdrawn is his discussion of the eighteenth-century Siberian journal *Irtysh*, which he suggests manifested strong sympathies for the French revolution.

A final problem concerns the author's failure to engage seriously the voluminous existing scholarship, Soviet or Western, that relates to his subject. The historiographic studies of S. L. Peshtich, for example, are not mentioned, and the monumental works of P. N. Berkov, Iu. M. Lotman, M. M. Shtrange, among others, are discussed only in passing.

This study, then, can be commended for a few of its contributions. Overall, however, it does not make a substantial contribution to Russian intellectual history.

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HANS ROGGER. *Jewish Policies and Right-Wing Politics in Imperial Russia*. Berkeley and Los Angeles: University of California Press. 1986. Pp. viii, 289. $25.00.

For the past twenty years Hans Rogger has been exploring anti-Semitism and right-wing politics in imperial Russia. By bringing together in this book six of his articles and a new introductory chapter, he demonstrates how thoughtfully and consistently he has considered his subject during this time. Although he has incorporated the most recent scholarship into his essays, he finds that it forces no change in his essential argument.

The theme that unites the essays is the disturbing question of why the virulent, Russian form of anti-Semitism resisted emancipation of the Jews so much more stubbornly than did Western anti-Semitism. He assures us that he has no easy answers. Fear and prejudice combined with objective factors. A relatively large ethnic group cramped into the small area of the Pale, the Jews were trapped in an underdeveloped economy with a peculiar social structure. Every action they took to better their lot rebounded against them. If they acquired land they were accused of taking bread from the peasants; if they went into commerce they were sucking their blood. If the Jews sought to assimilate they remained objects of discrimination; if they clung to their traditions they were denounced as alien. Anti-Semitism in Russia came close to racism, Rogger tells us, without ever completely embracing the idea. He dismisses the notion, popularized by Norman Cohen, that anti-Semitism was the outcome of a drive for national or religious unity. Yet he does not deny that profoundly irrational elements in the Beilis case, for example, drove government officials to experiment with anti-Semitism—as "a passion and conception of the world" in the words of Jean Paul Sartre. He agrees with Peter Struve that there was throughout Russian society "a strong feeling of repulsion for the Jews." The bureaucrats were suspended between their fear that the Jews would economically enslave the helpless peasant and their fear that pogroms unleashed by the peasants would mean chaos for Russia, "the black revolution." Their response was to avoid any extremes in legislation; they sought neither to assimilate the Jews nor to expel them but, rather, to muddle along. They passed a mass of legislation that followed "a recurrent pattern of harshness alternating with minor concessions" (p. 112). Even the moderates, who were mainly ministers of finance, sought to alleviate the worst restrictions on the Jews only for tactical reasons. The rise of anti-Semitic parties after 1905 fell short of breeding true fascists, Rogger insists. Russia had not yet been radicalized by a world war, its middle class was too weak to provide a respectable mass base for the fanatics in the Union of the Russian People, and the tsar on the throne made nonsense of *Führerprinzip*. As his introduction suggests, Rogger perceives the problem of Jewish emancipation as part of the general malaise of the Russian empire. The state was neither multinational nor unitary; behind the drive for industrialization lurked the shadows of anticapitalism; the rights of citizens clashed with the concerns of a paternalistic bureaucracy. The Jews suffered from every one of these contradictions.

Rogger admits that his study lacks an analysis of the popular sources of anti-Semitism that underlay high politics. One wishes he had tried his hand at it, for no scholar in the field commands as broad and balanced view of this controversial problem as he does.

ALFRED J. RIEBER
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SEYMOUR BECKER. *Nobility and Privilege in Late Imperial Russia*. Dekalb: Northern Illinois University Press. 1985. Pp. xiv, 259.

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In this well-researched book, based on unpublished archival records of the Special Conference on the Affairs of the Noble Estate (1897–1902) and on a wide variety of statistical data, Seymour Becker studies several crucial aspects of the history of the Russian nobility from 1861 to 1914: the changing relationship of the nobility to the land, the campaign waged by "self-appointed traditionalist defenders of noble interests," and "the emergence of a *class* of substantial landed proprietors out of the noble *estate*" (p. xiii).

At the beginning of his book Becker challenges the conventional view of the nobility as a social estate in the process of rapid decline. The nobility, he writes, "was not the passive victim of its own pathology and of external forces beyond its control" but was "an active participant in the process of adjustment to changing conditions" (p. 13). Becker admits that the majority of nobles divested themselves of their estates after 1861 but says they often did so because they could invest their capital more profitably in nonagricultural enterprises. Few nobles lost estates by failing to meet mortgage payments; in fact, they viewed mortgages from state lending institutions as easy sources of capital. Becker sees the minority of nobles who kept their land as increasingly "committed agrarian entrepreneurs" (pp. 53–54). His description of how nobles responded to market conditions is clear and convincing in most respects. His notion of landowning nobles as agrarian entrepreneurs, however, needs qualification. As he admits, at the close of the nineteenth century three-quarters of the nobility's arable and meadows was leased to peasants. One does not generally classify rentiers as committed entrepreneurs.

Chapters 3 to 7 treat traditional noble ideology and the campaign by traditionalists to halt the "decline of the nobility." These chapters sit uneasily with the first section of the book. After all, if no objective crisis affected the nobility, then why did the traditionalists lament the dangers threatening the first estate? Becker's answer to this difficulty is that the traditionalists represented only a tiny fraction of nobles "who found Russia's evolution away from hereditary privilege and toward legal equality both distasteful and alarming" (p. 57). The traditionalists tried to preserve the landed nobility by insulating it from market forces, restricting access to noble status, guaranteeing preferential treatment of nobles in government service, separating nobles from nonnobles in schools, and maintaining the local authority of nobles through institutions such as the land captains and noble marshals. In nearly every case the traditionalists failed to persuade the government to adopt their program. When they succeeded in winning changes in the law (as in the law of 1899 on entail), the vast majority of landowners ignored the changes. "By their lack of enthusiasm for schemes designed to protect them against social change," Becker notes, "the overwhelming majority of the remaining noble landowners demonstrated that they were not interested in becoming a fossilized social element, insulated from the free play of market forces" (p. 173). The issues discussed in these chapters have been treated at length by other historians of the nobility; Becker's contribution is to reinterpret the familiar material in light of his conviction that the nobility was not in decline.

In the third and weakest section of the book Becker examines noble politics from 1905 to 1914 and claims that in this period "estate consciousness" gave way to growing "class consciousness" on the part of large and intermediate landowners. "Interest-group politics based on class drew more response from substantial landowners than the schemes of the [traditionalist] *soslovniki* . . . ever had" (p. 173). Becker's conclusion rests on his understanding of the United Nobility as an economic interest group representing the 12 percent of the nobility whose families owned substantial quantities of land. In fact, the United Nobility was a more marginal and unrepresentative body than Becker imagines; furthermore, to describe its ideology as fundamentally different than that of the traditionalists of the 1890s is unjustified. Becker's concluding observations about the autocracy's culpability (and the nobility's relative lack of culpability) for the Russian revolution of 1917 are strange and unenlightening. Even less enlightening is the sudden reference to modernization theory as an explanation of Russia's historical course (pp. 174–75).

In the end, Becker's book is most valuable as a social history, establishing a quantitative portrait of the late nineteenth-century Russian nobility. As political analysis, the book is curious but unconvincing.

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BRIGITTE LÖHR. *Die "Zukunft Russlands": Perspektiven russischer Wirtschaftsentwicklung und deutsch-russische Wirtschaftsbeziehungen vor dem Ersten Weltkrieg.* (Historische Forschungen, number 11.) Stuttgart: Franz Steiner. 1985. Pp. 243. DM 84.

Brigitte Löhr's book is about German perceptions of Russia's economic condition during the decade before World War I. Relying chiefly on the writings of diplomats, state officials, financiers, and businessmen, she traces a broad development in the arguments of German experts who counseled against investment in Russia. Before 1909 they argued from Russian backwardness and inherent economic weakness; after 1909 they spoke of Russia's striking success at economic modernization but saw its growing strength as a threat to Germany and a pitfall for

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German investors. In early 1914, when Russia appeared to be facing an economic crisis, most doom cryers did not return to the earlier image of backwardness but instead focused on the apparent subservience of Russia to French influence. Russian power in crisis appeared even more dangerous to Germany than in boom times.

Löhr seems reluctant to extract sweeping conclusions from the tendencies she traces. On the contrary, by stressing the extreme diversity of interests that made up what are commonly called "reactionary" and "liberal" tendencies in Russian government, she creates considerable discord in these old harmonies. Following the lead of Walther Kirchner's seminal article (*Journal of Economic History* 1981 [41]: 361), she indicates that foreign investors held widely differing opinions on the Russian government. Different branches of industry supported different economic philosophies; individual firms within each branch did not always take the same view as the organs representing the industry as a whole. In 1908, for example, German chemical firms with branch plants in Russia actively supported "reactionary" tariffs, whereas German exporters of chemical products strove mightily in behalf of the "liberal" antitariff cause (pp. 81–82). In late 1905, following the publication of the October Manifesto, "revolutionary" workers and peasants in the south and southwest slaughtered Jews and burned out their businesses on an unprecedented scale, bringing virtually all trade to a halt. Russian credit fell to its lowest ebb (pp. 64–66). Somehow, however, the distribution network recovered. German investors, among others, continued to sell on credit in spite of all the solid reasons for staying clear of the maelstrom. In short, German investors in tsarist Russia cannot be usefully characterized by a consistent political ideology or measures in accordance with abstract concepts of economic interest.

It is refreshing to read a book on economic history in which the author does not labor to fit data into preconceptions. Löhr's study fails to explain German investment in tsarist Russia, but perhaps this is just as well. Perhaps capital investment is a much more mysterious phenomenon than we have realized. If Löhr's account does not make clear whether the tsarist government dominated Russia's creditors or vice versa, it is because the issue is far from decided. If Löhr cannot explain why German investors put so much capital into tsarist Russia in disregard of an abundance of expert, well-reasoned warnings, it is probably because no satisfactory explanation is as yet available. Löhr is to be praised, not blamed, for not offering generalizations.

GEORGE YANEY
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D. A. KOLESNICHENKO. *Trudoviki v period pervoi rossiiskoi revoliutsii* [The *Trudoviki* in the First Russian Revolution]. Moscow: Nauka. 1985. Pp. 317. 3 r.

In the past decade Soviet historians have paid greater attention to the "populist" parties, which were among the Bolsheviks' main rivals in the early twentieth century. Some of the best of these studies (notably V. N. Ginev's work on the Socialist-Revolutionaries [SRs] and N. D. Erofeev's on the Popular Socialists) have shown a degree of understanding of and sympathy for their subjects that was unthinkable for earlier Soviet historiography. D. A. Kolesnichenko's writings on the *Trudoviki* also fall under this category. The author of a doctoral dissertation and a number of articles on the *Trudoviki* in the First Duma of 1906, she has now extended her work into 1907 and the period of the Second Duma.

The State Duma, the quasi-constitutional legislative assembly created as a result of the tsar's October Manifesto of 1905, was elected on the basis of a complex indirect voting system, involving a property franchise, which nonetheless gave the peasantry the right to elect about 45 percent of the deputies. The government had apparently calculated—wrongly, as it turned out—that the peasants would vote for conservatives. In the First Duma a group of about one hundred deputies, the majority of them elected by the peasant constituencies, constituted the Labor Group (*Trudovaia gruppa* or *Trudoviki*). In the absence of any significant representation of the revolutionary and socialist parties, which had mostly boycotted the elections, the *Trudoviki* were the most substantial leftist faction in the Duma, and they quickly became the objects of competing political influence from parties both inside and outside the Duma. After the dissolution of the First Duma, the *Trudoviki* fought the elections to the Second Duma as part of a "left bloc," and in the Duma itself they formed a united fraction with the representatives of the All-Russian Peasant Union. The socialist parties, which had dropped their boycott, were represented in the Second Duma, but attempts to get the *Trudoviki*, the Popular Socialists, and the SRs to act together as a single populist bloc, let alone to form a left bloc including the Social Democrats, ended in failure. The land question was of particular interest to the peasant deputies. In the First Duma more of the *Trudoviki* supported the agrarian proposals prepared by the Popular Socialists (the Draft of the 104) than those of the SRs (the Draft of the 33). The SRs, however, succeeded in obtaining greater support from the *Trudoviki* for their proposal for land reform in the Second Duma than they had in the First.

Kolesnichenko's study of the *Trudoviki* shows their social composition and demands to have been more truly representative of the peasantry than those of

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either of the other populist fractions. The author sees them as the embryo of a genuinely peasant party—a status, however, that they failed to obtain in 1906–07, when they remained a group of Duma deputies, with no real organization in the country as a whole. The *Trudoviki* saw themselves as a nonparty and supraclass organization, representing the interests of "labor" in the populist sense of a broad alliance of peasants, workers, and intelligentsia. Kolesnichenko's interpretation of their significance, however, is naturally heavily influenced by Lenin's writings. The Bolshevik leader regarded the *Trudoviki* as the representatives of the "revolutionary-democratic" elements among the peasantry with whom the proletariat could form an alliance, and Kolesnichenko judges their actions from this Leninist persective. Within these limitations, however, the study is interesting and informative and is based on a wealth of archival material that has not previously been used by historians.

MAUREEN PERRIE
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S. A. KRASIL'NIKOV and V. L. SOSKIN. *Intelligentsiia Sibiri v period bor'by za pobedu i utverzhdenie Sovetskoi vlasti (1917–leto 1918).* [The Intelligentsia of Siberia during the Period of Victory and Consolidation of Soviet Power (1917 to Summer 1918)]. Novosibirsk: Nauka. 1985. Pp. 253. 2 r. 60 k.

This rewarding book, printed in only 1,350 copies and intended for historians, is concerned with the Bolsheviks and Siberian intellectuals from the fall of the monarchy to the summer of 1918, when the Whites took control of Siberia. Relying on published and archival sources as well as materials in private collections, S. A. Krasil'nikov and V. L. Soskin describe the reactions of various Siberian educated groups to the events of 1917 and 1918. The story is told in three phases: from February to October 1917, the October revolution, and the period from October 1917 until the summer of 1918.

Between the February and October revolutions most Siberian intellectuals opposed the Bolsheviks. The Siberians had "illusions of constitutionalism," meaning they believed the Constituent Assembly could avoid class warfare, and they supported the war. Only a few supported the Bolsheviks, but many, considering themselves above politics, professed neutrality. In this period the Bolsheviks sought to stimulate neutralism among the majority while attracting the "half-proletarian performers of mental labor" to their cause.

This same pattern persisted after Soviet government was established in October. But the Soviets could now freely use force against the "bourgeois bosses" who openly opposed Soviet rule. Further, the Soviets now desperately needed educated specialists to help them govern. Maintaining the flow of food from Siberia to European Russia was also essential. In this period the neutrality of the bulk of the intellectuals combined with fear of unemployment to induce them to work for or with the Soviet authorities. Between 1917 and the following summer Soviet authority was established, only to be overturned by the Czechoslovak revolt.

Krasil'nikov and Soskin furnish much information on the Siberian periodical press in 1917 and 1918, on Siberian autonomism (*oblastnichestvo*), and on the attitudes of groups in Siberian cities. The book is not, however, faultless. The sociology is distorted to fit Soviet preconceptions, so that Mensheviks and Socialist Revolutionaries become bourgeois, and professors, lawyers, and engineers become "bourgeois bosses." Refusal to recognize Soviet power is labeled "sabotage."

But there are encouraging signs. A book by the anti-Communist G. K. Gins, a member of A. V. Kolchak's government, is openly cited. In general, books are listed by author and title rather than as items in archival *fonds*. Applying a technique rare in Soviet scholarship, the authors use content analysis to ascertain the political orientation of Siberian periodicals. This book is a valuable contribution to historical understanding.

PATRICK R. TAYLOR
University of Tennessee, Martin

ALFRED EISFELD. *Deutsche Kolonien an der Wolga 1917–1919 und das Deutsche Reich.* (Veröffentlichungen des Osteuropa-Instituts München, Geschichte, number 53.) Wiesbaden: Otto Harrassowitz. 1985. Pp. 177. DM 44.

Alfred Eisfeld's study focuses on the Volga Germans during two tumultuous years between the outbreak of revolution in Russia in February 1917 and the creation of an autonomous Volga German district in April 1919. Eisfeld's effort is welcome because this theme touches on complex issues during a crucial phase of the revolution and has received little scholarly attention.

Eisfeld proposes to determine who among the Volga Germans were bearers of the revolution, the nature of their motives, the impact of economic conditions on colonist attitudes, and the role the colonists played in the objectives of the German empire. Yet his is a standard political history of the Volga Germans during the revolution and Civil War. Eisfeld concentrates on colonists' conferences, missions to Berlin, and discussions with Soviet authorities. In Eisfeld's judgment, despite tsarist at-

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tempts at russification, Volga Germans turned to Germany only after revolutionary confiscation of grain, livestock, and property. The collapse of the German empire dashed hopes for resettlement or occupation of Volga German districts by the German army. Ultimately, Volga Germans had no choice but to negotiate autonomy on the Bolshevik regime's terms.

Eisfeld often subordinates discussion of Volga German concerns to an analysis of activities and aspirations of German colonists elsewhere in Russia. Such broader considerations were probably unavoidable; however, Eisfeld covers neither the narrower Volga German topic nor its wider setting adequately. Eisfeld says that his more inclusive approach resulted from failure to gain access to relevant Soviet and East German archives. This also explains why the author concentrates on political analysis at the expense of the important social issues raised in his introduction.

The text is appended with a series of well-selected documents. A map would have been useful.

BENJAMIN L. BENFORD
Tuskegee University

PHILIP POMPER, editor. *Trotsky's Notebooks, 1933–1935: Writings on Lenin, Dialectics, and Evolutionism.* Assisted by YURI FELSHTINSKY. New York: Columbia University Press. 1986. Pp. viii, 175. $25.00.

The two notebooks and clutch of additional notes that constitute the foundation of this book are modest in bulk, roughly seven thousand Russian words. These materials from the Trotsky papers at Harvard University are meticulously presented in both the original Russian and English translation, with an introductory description of their physical format, footnotes in Russian on peculiarities of the text, such as grammatical errors and color underscoring, and explanatory footnotes in English on Trotsky's allusions, mainly to people. Scrawled in pencil, with many abbreviations, the notebooks presented problems of transcription, which Yuri Felshtinsky has solved. Of about thirty-eight pages of the notebooks in translation, about eight concern Lenin and history—preliminary jottings for Trotsky's intended but unwritten life of Lenin. The rest consists of a scattering of thoughts on dialectical philosophy and science, which in Trotsky's mind formed a single category. Philip Pomper shows that this was inspired largely by Max Eastman's challenge to Trotsky's Hegelian convictions.

Although this archival material by itself would interest only a few specialists, Pomper's introduction, which occupies almost half the volume, endows the notebooks with high intellectual appeal. A master of the primary and secondary literature on Trotsky, he places the notebooks in their biographical context and offers an analysis of the revolutionary's "dialectical style" in contrast to that of some of Trotsky's Russian Marxist contemporaries. Among Pomper's observations concerning matters biographical, I found particularly apt the suggestion that Trotsky was attempting to characterize himself in his descriptions of Lenin's strengths. On dialectical style Pomper sees Trotsky as emphasizing the "catastrophic principle," in contrast to Lenin's espousal of "flexibility."

Without losing his critical balance, Pomper is highly respectful of Trotsky's intellect, which has justifiably impressed many observers, Trotsky among them. Concerning Trotsky's vanity and his notebooks on dialectics and evolutionism, there is one basic question (perhaps too elementary to Trotsky and Pomper to prompt either of them to address it at length) that I wish Pomper's essay had explained for me. Did Trotsky see the dialectic as a heuristic device only? Or did he believe that, with his self-taught knowledge of science, based partly on newspapers, he could discover how the dialectic works as a kind of "unified field theory," embracing all the natural and social sciences?

ROBERT H. MCNEAL
University of Massachusetts

NEAR EAST

RALPH S. HATTOX. *Coffee and Coffeehouses: The Origins of a Social Beverage in the Medieval Near East.* (Near Eastern Studies, number 3.) Seattle: Department of Near Eastern Languages and Civilizations, University of Washington; distributed by University of Washington Press, Seattle. 1985. Pp. xii, 178. $9.95.

The discovery in Ethiopia or the Yemen that the coffee bean could be used for brewing a pleasant and harmless hot drink was a noteworthy service for human society. According to documented evidence, it happened in the fifteenth century. From Arabic sources and reports by European travelers in the Near East, Ralph S. Hattox has put together the available information and discussed its significance in great detail. In particular, he wishes to point out an important transformation—if transformation is not too strong a word—in Near Eastern society as the result of the introduction of coffee. In the Muslim environment coffee had three great virtues. It was a stimulant that caused no real physical effects (and those it did were mostly in the imagination), in contrast to wine, which was outlawed by the Koran. It was conducive to socializing, in contrast to other drugs that were, or were considered, introverting and normally tended to make their users shun

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With journals, monographs, theses, and official publications as major support, the book is based predominantly on newspapers. They are, of course, good raw material for urban history. They make rather tedious reading, however, if a writer frequently repeats verbatim stories about city life instead of forging them into a conceptual order that sustains a theme. The language of these stories may be the "stuff" of history, as the author stresses in the preface, but the distance between the "stuff" of history and historical writing is probably as large as the distance between the fact as event and the fact as history. Instead of exposing the complexities of urban life, the tales merely add local color.

The book gives quite a different impression when McComb deals with Galveston's ecology. The opening sketch of the geographic setting, with its assessment of geological features, meteorological conditions, in particular hurricanes, corresponds well with the description of the great storm of 1900 and the discussion of the building of the seawall that began three years later. Today, the city, once considered the major port between New Orleans and Veracruz, has lost to its old rival Houston almost all areas that make a city great, except its beach. That fact lends a special dimension to this account of Galveston with its emphasis on the role of nature in the development and decline of an American city.

GUNTHER BARTH
University of California, Berkeley

JEAN HEFFER. *Le Port de New York et le commerce extérieur américain, 1860–1900*. Paris: Publications de la Sorbonne. 1986. Pp. ii, 568. 100 Fr.

This well-written econometric maritime history of New York picks up where Robert G. Albion's pioneering work left off, 1860. Jean Heffer has combined his substantial background in both U.S. and economic history to produce a very readable account of the period 1860 to 1900. One should have some facility with French and not be put off by statistical data or examples of mathematical formulas as used to determine New York's portion of exports from customs statistics found on pages 182–83, and tables 6–8, on pages 195, 197, and 200. These are just a portion of the 72 graphs and 19 tables that provide additional data and are explained in the text as well. The footnotes are at the end of the volume, making reference to them a bit awkward, although some notes appear with the graphs and tables. The list of these and the table of contents appear at the end of the volume. A short bibliographic essay points out the many statistical sources used, most of which are government publications. Unfortunately, the index does not subdivide items such as fruits, grains, or animal products. With some effort, however, one can find information on a specific item. There are extensive page references for Baltimore, Boston, New Orleans, Philadelphia, and maritime lines.

The first fifth of the book deals with the impact of the Civil War on New York's growth and its advantages in trade, particularly exports. The remainder deals with trade after the war. Heffer provides a fine chapter on the Erie Canal and much material on the impact of railroads. The author makes it quite clear why New York pulled ahead of its rivals. New Orleans lost out in the Civil War, and San Francisco had only a limited trade compared to the transatlantic. One might question the emphasis the author places on the role of the Civil War in the rapid decline of the American merchant marine and its failure to recover (p. 81). More emphasis should have been placed on developments of the 1850s, the flight from the flag, and increased foreign competition. Nevertheless, this book is an excellent addition to nineteenth-century maritime and urban history.

MARY EMILY MILLER
Salem State College

LAWRENCE A. CLAYTON. *Grace: W. R. Grace and Company; The Formative Years, 1850–1930*. Ottawa, Ill.: Jameson. 1986. Pp. xiii, 403. $22.50.

The history of W. R. Grace and Company offers the opportunity for an inside study of an unusual firm. Although most big American enterprises in the late nineteenth century concentrated on the domestic market and on businesses closely related to the Industrial Revolution, Grace, which by 1917 was the fifty-second largest U.S. industrial, was built on traditional commerce and shipping in overseas enterprise, especially in Peru and along the west coast of South America.

Unfortunately, the book does not fulfill its opportunity despite the use of large collections of documents at Columbia University and in the company's archives. The treatment lacks a clear focus, attempting to be both a company history and an analysis of modernization and dependency in a developing country. The book skims important parts of the firm's history between 1850 and 1930 and leaves unfinished its study of the company's operations in Peru yet devotes two chapters to such unrelated topics as William Grace's political career in New York and the causes and conduct of the war between Peru and Chile of 1879–81.

Lawrence A. Clayton is at his best when documenting the rise of the small enterprise from its Peruvian roots as a mixture of timing, talent, and energy. William Grace forsook impoverished Ireland and his family's agricultural heritage to become

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a partner with a ship chandler in Peru in 1854. As Peru's international commerce grew with its guano trade, Grace aggressively expanded the shipping business and seized new opportunities offered by the developing economy—importing, exporting, and supplying railroad contractors. As his businesses increased in number and scope, Grace imitated earlier general merchants by recruiting family members and close associates in a series of loosely linked partnerships in key ports such as New York, San Francisco, London, and Valparaiso. At the founder's death in 1904, W. R. Grace and Company was perhaps the largest importer and exporter in South America. Under William's son, Joseph P. Grace, the company maintained its traditional lines and expanded earlier investments into nitrate mining, textile manufacturing, the cultivation of sugar cane, worldwide shipping, and, eventually, aviation.

As his subject grows bigger and more complex, Clayton's account becomes more uneven and unclear. The author's reliance on anecdote and detail provides a vivid, rich narrative that helps document the emerging rivalry of the United States with England for the South American trade, but, without a framework for big business or multinational enterprise, his picture is incomplete. Clayton virtually ignores the work of Alfred Chandler and Mira Wilkins. A key turning point such as Grace's shift from commerce to manufacturing and services receives only brief treatment. The final chapter, which focuses on a period (the 1920s) rather than on the enterprise or on its Peruvian operations, is rushed and fragmented.

In sum, Clayton's book supplies a wealth of data about an important and little-studied company. The story's significance and framework, however, are left to the reader or to another study.

CHARLES W. CHEAPE
Loyola College

WALTER L. BUENGER and JOSEPH A. PRATT. *But Also Good Business: Texas Commerce Banks and the Financing of Houston and Texas, 1886–1986*. College Station: Texas A&M University Press. 1986. Pp. xvi, 450. $29.50.

"Public" and "corporate" history is a burgeoning field, as attested by the number of new books appearing. Whereas most commissioned works, whether by the specific limitations of the contract, the wishes of the employer, or the inclination of the author, focus on the business or corporation under study, several more recent studies have broken free of those constraints to include broad discussions of business history, management theory, economic analysis, and regional development. The present book exemplifies this recent trend by placing the history of Texas Commerce Bankshares into the context of economic growth in Houston and, more generally, Texas.

Analyzing the changing demands of financing the Texas economy, Walter L. Buenger and Joseph A. Pratt follow this bank holding company (now with dozens of former unit banks under its corporate umbrella) from its birth in 1886 to the present. First, the bank concentrated on the cotton trade, then shifted to making oil loans, and finally, with the decline in oil prices in 1980s, diversified to emphasize real estate and business lending, especially outside the Houston metropolitan area. Like many new books about state banking, this book scrutinizes the impact of regulation on Texas's financial institutions, especially on its antibranch banking law. Texas Commerce, if anything, provides a blueprint for achieving long-term growth while evading, avoiding, or skirting nonsensical, poorly conceived, stifling, and contradictory government regulations. Several broader works have dealt with the harmful effects of the numerous state antibranching laws in the United States, and to their credit the authors do not harp on this topic. But some of their examples will undoubtedly find their way into standard business history lectures. For instance, in 1964, when Texas National Bank and the National Bank of Commerce merged, the antibranching law required that the two merged banks be combined at a single location, even though the buildings stood across the street from each other. The law, however, provided that a drive-in window across the street was permissible if connected to the parent bank by a permanent tunnel. So, at the expense of 50,000 dollars, a tunnel was constructed joining the two banks; it was used only one time and thus met the letter of the law.

Structurally, the authors' approach to a corporate history, alternating chapters on the company's growth with chapters on general regional development, is of limited success, because a great deal of material is repeated. Likewise, the large inserts in each chapter, usually concerning important individuals in the bank's past, suggest that the authors had a pile of wonderful material that otherwise did not fit in the flow of their narrative. But any veteran of corporate historical writing will realize that the authors had to give major executives their due. Although this book is pregnant with statistics on lending, assets, buildings, and growth, the tables sometimes overburden the reader. Nevertheless, inclusion of these figures makes this book indispensable to future research on the economy of Texas and on western banking.

As for content, equally important as the abundance of financial data are two other points analyzed in detail by the authors: the change in style of management from conservative and personal to aggressive and national; and the concern by man-

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agement for stability both in succession of leadership and in lending and asset growth. In the former case, although the authors pay due homage to Alfred Chandler, comparisons to a few existing state or regional banking histories such as Lynne Doti's work on California, James Smallwood's book on Oklahoma banking, or Eugene White's thorough study of bank regulation would also have been useful. As for the latter theme, the bank only suffered in one major crisis and otherwise effectively achieved stability in a regional economy that featured several major disruptions.

Finally, public and corporate historians will see from this book that scholarship need not be sacrificed when producing a commissioned history (pay particular attention to the authors' contract, outlined in the introduction). Although the Texas Bankshares family of friends, customers, employees, and directors who receive a copy of this as a Christmas gift will not find it appropriate "coffee table" material, they ought to appreciate it. This is good history.

LARRY SCHWEIKART
University of Dayton

SERGE RICARD. *Theodore Roosevelt et la justification de l'impérialisme*. Aix-en-Provence: Université de Provence. 1986. Pp. 417.

This French scholar's study of Theodore Roosevelt is of genuine interest to American students. Based on an extensive historiographical review of books on Roosevelt, supplemented by such sources as the president's letters and writings, Serge Ricard discusses the American statesman's racism, militarism, and imperialism in historical context.

A conservative nationalist and domestic reformer, Roosevelt reflected the roots of American history and contemporary developments in his actions. He tenaciously implemented his conviction that the superior races, such as the Anglo-Americans, had a moral obligation to lift up the savage barbarians and show them the way to democratic civilization and self-respect. He was equally convinced that only militarily strong nations could keep the peace and survive. Perhaps most significant was his realization that a balance of the existing imperialisms in the world had by the turn of the century become imperative for global peace. He rose to the level of a statesman by making the United States an active participant in world affairs. For the sake of security and peace, he overcame congressional and public opposition to his break with traditional isolationism with the determined expansion of the powers of the executive.

A supporter of the Monroe Doctrine and the Open Door, he was influenced and strengthened by such eminent thinkers as Captain Alfred Thayer Mahan and Frederick Jackson Turner. But, whereas some critics and defenders of imperialism attribute its main justification to economic motives, Ricard points out that Roosevelt did not fully share this viewpoint, even though he did not exclude it altogether. Roosevelt's prime justification was to civilize and democratize "backward" regions. At the same time, he was aware of the rapidly growing challenges of financial and industrial capitalism.

Although one should not expect the author to say much that is new, he is to be congratulated for his clearly presented panoramic picture. His study is not only thorough and comprehensive but also balanced. As the author shows, Roosevelt, despite his aggressive exploits and manipulations in Cuba and Panama, displayed much tact and diplomacy in the Moroccan Crisis and the Russo-Japanese War.

Finally, Ricard credits Roosevelt with laying the foundation for America's future foreign policy. Only after World War II was the nature of American imperialism modified.

HENRY BLUMENTHAL
Rutgers University

BETTY SPEARS. *Leading the Way: Amy Morris Homans and the Beginnings of Professional Education for Women*. (Contributions in Women's Studies, number 64.) New York: Greenwood. 1986. Pp. xiv, 179. $39.95.

This is a more sentimental than analytic study of Amy Morris Homans, a pioneer in the field of physical education for women. Betty Spears relates Homans's career to critical developments in the history of sport and physical education for women. In the post–Civil War decades, when physicians decried the ill-health of women, Homans, with the financial backing of Mary Hemenway, founded the Boston Normal School of Gymnastics "to promote the health and vigor" of young women. By 1910 Homans had become convinced of the importance of making exercise and sport acceptable activities for college women; she promoted the merger of the school with Wellesley College and the building of the Mary Hemenway Gymnasium there. On its completion, Homans became the first director of the physical education program at Wellesley. As the program gained prominence, Homans lobbied for a master's degree in hygiene and physical education as a step toward her final goal: making physical education a profession for college-educated women.

Despite Homans's considerable accomplishments, documenting her life was a formidable task. Homans, determined to elude would-be biographers, requested that her correspondence and personal papers be destroyed; as a friend later recalled, she insisted that "if what I have done is worth anything,

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it will live. If it is of no use, let it die with me." As Spears sadly learned, her request was fully honored. As a result Spears was forced to reconstruct Homans's career from a few published speeches and the memories of former students. Unfortunately, the material is too thin to sustain a full-length biographical study. We miss a sense of the personal relationships that sustained this "reserved, well-mannered, indomitable New England lady," the reasons for her choice of career, and the links between her private and personal interests. In the end, this book is more successful at charting the history of physical education for collegiate women than in explaining the life choices of Amy Morris Homans.

SHEILA M. ROTHMAN
Columbia College for Physicians and Surgeons

GARRICK BAILEY and ROBERTA GLENN BAILEY. *A History of the Navajos: The Reservation Years*. Sante Fe, N.Mex.: School of American Research; distributed by University of Washington Press, Seattle, Wash. 1986. Pp. xx, 360. $30.00.

With a population exceeding one-hundred-fifty thousand, the Navajos are the largest Indian tribe in the United States. The tribe has grown in size and importance over the last century since the days of the Bosque Redondo exile when the Diné numbered as few as ten thousand. If one considers that so many American Indians today have lost their tribal identities, the Navajos seem to constitute an Indian success story. The persistence of Navajo culture is the central concern of Garrick Bailey's and Roberta Glenn Bailey's book.

The authors offer a simple explanation: Navajo history unfolded in a historical and cultural milieu almost unique among Native Americans. First, the tribe's geographical isolation removed them from the major impact of Euro-American settlement. But more important than geographical remoteness was Navajo social structure, based on the extended family or clan. The kinship system served as a cultural bulwark against change, regardless of the success or failure of the tribe's economic and political leadership.

This book is an economic history of cultural persistence. Despite the work's title, it is not a comprehensive tribal history, such as Peter Iverson's *The Navajo Nation* (1981). Nevertheless, the authors provide useful information on such important topics as herd size and per capita income.

Given the limits that the authors imposed on themselves, they effectively marshal evidence for their thesis of cultural persistence. But a problem exists in their research. As yet, Navajo history between 1868 and 1913 has not been examined in depth, and the authors of the work performed only cursory research in comparison to studies such as Donald Parman's *The Navajos and the New Deal* (1976).

Historians of the Navajos and other tribes will find the book to be a valuable contribution to scholarship, but the conclusion seems open to disagreement. The authors argue throughout their work that cultural transformations have come gradually to the Navajos. Slow change has permitted Navajos to persist and flourish. But, in recent decades, the pace of technological and economic transformation has quickened. With wage labor becoming increasingly important, the authors believe the nuclear family will come to dominate and "the cohesiveness and moderating influence of the extended family will be threatened. . . . We are beginning to witness just such a change," they write, "if the extended family vanishes as an institution, continuity in Navajo culture change will vanish with it" (p. 297). This sounds alarming, but, before predicting the demise of the Navajo way, we should note that ample evidence in Navajo history supports the opposite proposition. The Navajos have never been as static as they are portrayed in this work. Change has always been a part of Navajo culture, and indeed Navajo history seems to be based on frequent economic and cultural changes, despite which the Navajos have retained their identity. Iverson concluded *The Navajo Nation* by stating that "if their past and present teach us anything, it should be not to underestimate their strength, resilience, and persistence" (Iverson, p. 226). These traits have dominated the Navajo past and should prevail in the future against cultural extinction.

GERALD THOMPSON
University of Toledo

H. ALLEN ANDERSON. *The Chief: Ernest Thompson Seton and the Changing West*. College Station: Texas A&M University Press. 1986. Pp. xii, 363. $35.00.

WARREN L. HANNA. *The Life and Times of James Willard Schultz (Apikuni)*. (Based in part on a manuscript by Harry C. James.) Norman: Oklahoma University Press. 1986. Pp. xviii, 382. $24.95.

Ernest Thompson Seton (1860–1946) and James Willard Schultz (1859–1947) were two significant and remarkable figures who influenced twentieth-century attitudes about the wilderness and about native Americans. Prolific writers who reached their peak of success and influence during the Progressive era and in the 1920s, Seton and Schultz were neglected in the 1950s, but both enjoyed a revival in the late 1960s and in the 1970s as concern with environmental issues and interest in Indians grew.

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Born in England and reared in Canada, the gifted Seton became an artist, acclaimed naturalist, writer of animal stories, and social commentator. His lectures, sketches, stories, and books featuring nature and animals reached hundreds of thousands of people at the very time that industrialization and urbanization were changing the natural environment of North America. Seton worked closely with other naturalists and environmentalists of his time, and together they influenced such powerful politicians as Theodore Roosevelt.

H. Allen Anderson notes that the short stories of Seton called attention to nature just as the works of Frederic Remington or Owen Wister prompted readers and viewers to perceive images of the cowboy and the West. Yet a sharp difference separated the characters of Seton from Wister's and Remington's heroic cowboys, who often contested nature. "These personifications of materialism and industrialism [the cowboys] seek to devour the pristine wilderness and the natural order of things that [Seton's] animal heroes represent," Anderson writes, "The cattle and the sheep of the Western range are seen merely as 'four-legged cash,' and the men who herd them as vanguards of urban-industrial society, with all its ills, soon to follow" (p. 98).

Seton's profound feelings about nature clearly governed his view of the Indians. He believed that Indians lived in harmony with nature, whereas non-Indians did not; indeed, "Seton came to believe that the Indian was not only attuned to nature but that he *was* nature" (p. 185). Seton founded the Woodcraft Indians to instill into boys a respect for nature, and, by 1910, Woodcraft Indians was the largest youth organization in the United States. After much debate and factionalism Seton and his organization helped found the Boy Scouts of America.

Seton's interest in Indians was genuine, and he wrote a comprehensive book on the Indian sign language. Still, he viewed Indians not as real people with unique cultures but as a "type" who fit his notions about humanity's place in nature.

In sharp contrast to Seton, who knew something about numerous Indian groups, was James Willard Schultz, who knew one tribe intimately. A native of New York State, Schultz, at the age of seventeen, went to Fort Benton, Montana, in 1877. There he became a trader to the Piegan, one of the three Blackfeet tribes. Schultz eventually married a Piegan woman, learned the Blackfeet language, and, as a trader, rancher, or guide and outfitter, lived near the Piegan for nearly three decades. After the death of his wife he frequently returned to the Piegan and other Blackfeet for the rest of his life.

Schultz began writing to support his family, and (like Seton) he found eager publishers and an avid audience for his many serials, books, and articles. Certainly, his most popular and enduring book was *My Life as an Indian*, published in 1907. Best known as a writer about Indians, Schultz (or "apikuni," his Piegan name) was not a naturalist, yet he called national attention to the area that became Glacier National Park.

Schultz and Seton attracted the attention of those concerned with nature and with Indians. Both men wrote for George Bird Grinnell's influential magazine *Forest and Stream*, and both were well acquainted with biologist and naturalist C. Hart Merriam, namesake of Schultz's son. Schultz did much of the research for Grinnell's book on the Blackfeet.

Seton and Schultz were active in efforts to help the Indians. Several times Schultz tried to alleviate the starvation that gripped the Piegan Reservation in Montana. Schultz was a charter member of the Indian Welfare League, which worked with other organizations to secure legislation making Indians citizens of the United States. It passed in 1924. Seton was involved with several groups dedicated to Indian causes. Seton supported the American Indian Defense Association's fight against the attempts of Secretary of the Interior Albert B. Fall to open reservation lands to leasing for oil and gas. In 1932 Seton opened his "College of Indian Wisdom" in Santa Fe, New Mexico; he enthusiastically supported John Collier, New Deal Commissioner ofIndian Affairs, and the passage of the Indian Reorganization Act (or the Wheeler-Howard Act) of 1934.

The Chief: Ernest Thompson Seton and the Changing West will become the standard biography of Seton. Anderson has thoroughly researched Seton, and *The Chief* has a clearly organized narrative. For the most part, the book is well written and readable, although Anderson occasionally resorts to clichés.

Unhappily, Warren L. Hanna's *The Life and Times of James Willard Schultz (Apikuni)* is not as felicitous as *The Chief*. Hanna's book is choppy, and the narrative is difficult to follow because it often moves forward or backward twenty or more years in time, with little warning to the reader. Much of the material in the book is purely anecdotal, and some chapters ("Apikuni and His Addictions" or "Apikuni and His Pets") should have been severely trimmed. The author also does not clearly place Schultz and his writing into the social and intellectual context of his times, and readers have to rely on their own knowledge of the period and its issues to appreciate the significance of Schultz. Despite these problems, the inherent interest and importance of Schultz's life should hold the attention of most readers.

The precise impact of either Seton or Schultz is difficult to evaluate. Seton's art, lectures, writings, and organizations and Schultz's articles and books touched a generation of Americans and their lead-

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ers. Certainly, Schultz and Seton helped lay the foundations of attitudes about nature and about Indians that bore fruit in the legislation of the New Deal and the environmental and social concerns of the 1960s and 1970s.

JOSEPH C. PORTER
Center for Western Studies
Joslyn Art Museum
Omaha, Nebraska

DAVID W. DETJEN. *The Germans in Missouri, 1900–1918: Prohibition, Neutrality, and Assimilation.* Columbia: University of Missouri Press. 1985. Pp. ix, 244. $23.00.

This is a competent work by an attorney from a family of German-American lawyers who have represented Germans and German-Americans in their legal affairs for three generations. As he states in his preface, David W. Detjen initially intended to "write on the prosecution of various leaders of the [German-American Alliance] under the Espionage Act of 1917" (p. vii). His earlier training in history combined with his experience as a practicing attorney would have eminently qualified him for such an endeavor, as he clearly demonstrates in his best, final chapter dealing with the trial of Dr. Charles H. Weinsberg, the leader of the German American-Alliance in Missouri. Had he continued in this vein, a closer analysis of the judicial system's response to the anti-German hysteria of that period might have corrected the impression given by some writers that almost all U.S. judges and juries presumed Germans guilty until proven innocent.

Instead, Detjen chooses to trace the history of the alliance in Missouri from its beginnings at the turn of the century to its demise in 1918. As had others before him, he realized how "inextricably" the activities of the alliance during the period of neutrality were "tied" to the "temperance and prohibition campaigns" preceding it (p. vii). Accordingly, the work divides roughly into three parts: background information on German-Americans in St. Louis and Missouri and the organization of the German-American Alliance; the period of neutrality from 1914 to 1917, in which the experience the alliance had gained in its agitation against prohibition on the state level was turned toward convincing the U.S. Congress to keep the United States out of the war; the U.S. entry into the war and the subsequent demise of the alliance, culminating in the trial of Dr. Weinsberg.

Aside from the last chapter, the book is valuable only insofar as most of Detjen's findings corroborate for Missouri what other scholars have already found elsewhere: German-American communities were "doomed in any event" (p. 186). The lack of new immigrants from Germany and the rapid assimilation of the second generation saw to that. World War I only hastened their demise, in which "the German American Alliance had played a sad [but] significant part" (p. 186).

This is not to imply that his work is devoid of any valuable contributions—his analysis of the activities of the German-American Alliance in Missouri is a case in point—but rather that they are lost in a book-length study covering much ground already plowed over by others such as Clifton J. Child, John Hawgood, Frederick Luebke, and Audrey Olson. Because of this, Detjen's work conveys an impression of *déjà vu* that is not altogether deserved.

GUIDO A. DOBBERT
Youngstown State University

WILLIAM E. ELLIS. *"A Man of Books and a Man of the People": E. Y. Mullins and the Crisis of Moderate Southern Baptist Leadership.* Macon, Ga.: Mercer University Press. 1985. Pp. xi, 228. $18.95.

In this concise and clearly written study William E. Ellis contends that Edgar Young Mullins, president of Southern Baptist Theological Seminary in Louisville, Kentucky, from 1899 until his death in 1928, was the most important Southern Baptist theologian and public figure of the twentieth century. The argument is persuasive. Mullins authored a dozen books and scores of articles on systematic theology, acted as a learned and urbane spokesman for Southern Baptist concerns in the North and in the Baptist World Alliance, did more than his share to block Baptist fundamentalists from pushing an antievolution bill through the Kentucky legislature in the early 1920s, and, in the last two years of his life, stepped into the political arena to fight for Prohibition and Herbert Hoover.

The book should be useful to anyone interested in the development of theological education in the United States, the history of Baptists in the South, or precedents for conservative Protestants' current involvement in national politics. Moreover, it offers several surprises. One is that Mullins, who always stressed the primacy of supernatural religious experience in the life of the believer, had a rather intellectualized religious conversion himself and does not seem to have been particularly susceptible to religious emotions any time in his life. A second surprise is the extraordinary amount of time and energy Mullins invested in fighting off the instrusions of Baptist fundamentalists—who of course held most of the financial chips—in the life of the seminary. Although Ellis argues that Mullins sought to be a moderating influence in the Southern Baptist Convention, one gains the impression that Mullins's first concern was to be not a mediator but

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a defensive guard, perennially struggling to protect the seminary from conservative forces determined to keep newer critical understandings of Scripture and church history out of the institution.

Ellis tells us a great deal about the seminary's financial difficulties and the public controversies Mullins weathered, but he says little about Mullins's private life and nothing about the ways that Mullins's private life may have influenced his public career. More serious is the virtual absence of attention to Mullins's theological work. Ellis notes that Mullins strove to integrate Friedrich Schleiermacher's emphasis on individual religious experience, William James's stress on pragmatic tests for religious truth, and Borden Parker Bowne's affirmation of the ultimately personal nature of reality into the framework of traditional evangelical thought. But Ellis does not tell us how Mullins executed that considerable feat, nor does he make any effort to assess the enduring value of Mullins's work. All of this is to say that a biography of Mullins has long been needed, and, although this one is a step in the right direction, much remains to be done.

GRANT WACKER
University of North Carolina

ROBERT PAUL BROWDER and THOMAS G. SMITH. *Independent: A Biography of Lewis W. Douglas.* (Borzoi.) New York: Alfred A. Knopf. 1986. Pp. xiii, 491. $30.00.

The scion of a wealthy copper mine owner, Lewis Douglas was raised in Arizona, educated at Amherst, and elected to his state legislature before he was thirty. He became an expert on water rights and irrigation and during the 1920s was elected as Arizona's sole delegate in the House of Representatives. He served four terms, and in 1933 Franklin D. Roosevelt appointed him budget director, a position he held until the next year. He became vice chancellor of McGill University and then president of Mutual of New York insurance company until World War II, when Roosevelt asked him to direct the War Shipping Administration. Perhaps his most important position was his last in government service, as ambassador to Great Britain from 1947 to 1950, where he was concerned with issues such as the Marshall Plan, Berlin Blockade, Palestine, and creation of the North Atlantic Treaty Organization.

Robert Paul Browder and Thomas G. Smith have produced a traditional biography. Those looking for a psychohistorical analysis of a personality will not find it in this book, but readers will discover a fine job of examining the policies and views of Douglas. The authors demonstrate that their subject, although elected as a Democrat, was a conservative, who after 1932 rarely voted with his party. He championed states' rights, balanced budgets, and low tariffs and supported the depression policies of Herbert Hoover. Douglas was stubbornly conservative as Roosevelt's budget director, and, while unemployment soared and money deflated, he urged spending cuts and worried about inflation. At odds with the president, he resigned and became a critic of the "administration's design"—"state socialism" and the "collective system" (p. 123). Nevertheless, Douglas's administrative skills were needed during the war, and during the cold war he became an effective ambassador. He preached Anglo-American unity, and, although he supported the British concerning Palestine, he advocated the European policies of President Harry S. Truman.

This is a good biography, and one of its strengths is the judicious research conducted by these two scholars in virtually all sources in the United States and Britain. Browder and Smith have examined the private papers of Douglas, oral histories, newspapers, secondary studies, and British and American official documents and have conducted interviews on both sides of the Atlantic.

The book's weakness, if any, is typical of a biography—the authors adore their subject. It is difficult to believe that Douglas—or anyone—always was so charming, shrewd, honest, and considerate. On rare occasions the authors' statements contradict the quotations of their subject. For example, Browder and Smith contend that Douglas was "certainly free from prejudice" (pp. 15–16) but later report his anti-Semitic statements about "Hebraic influence" in the New Deal (p. 111). Furthermore, the authors claim that Douglas supported academic freedom at McGill University but then report that he attended lectures of socialist professors and was instrumental in firing those who supposedly were teaching the "collectivist philosophy" (pp. 136–37).

This is a favorable biography; it also is an important book. Historians will use it as the standard reference on the policies and career of Lewis Douglas.

TERRY H. ANDERSON
Texas A&M University

DONALD L. SMITH. *Zechariah Chafee, Jr.: Defender of Liberty and Law.* Cambridge: Harvard University Press. 1986. Pp. x, 355. $25.00.

Surveying the life of one of the nation's most eminent legal scholars, Donald L. Smith's biography of Zechariah Chafee, Jr., offers insights into the evolution of First Amendment rights in twentieth-century America and is grounded in extensive research into primary sources and secondary studies. As biography, this monograph offers few penetrating insights into the character of the man who became one of

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the most prominent and consistent proponents of freedom of speech and who opposed efforts, justified under the guise of national security, to restrict this right. Smith's study is more an intellectual biography than an account of Chafee's personal life and the influences that determined his interest in a career as legal scholar and First Amendment spokesman. Furthermore, Smith eschews a chronological organization, and the result is a disjointed narrative that fails to explain the effects of changes in American values and politics on Chafee's influence and positions.

Smith's narrative is uneven. The author recounts the major events of Chafee's controversial career: his opposition to the Palmer Raids and to the Justice Department's surveillance policy during World War I and the efforts during the early 1920s of a conservative Harvard alumnus, Austen Fox, to purge him from the Harvard faculty because of his challenge to the department's surveillance activities; Chafee's influence as teacher and publicist on his students and the legal community; Chafee's role in drafting the Interpleader Act of 1936; Chafee's appointment to the United Nations Subcommission on Freedom of Information and of the Press and his efforts to advance human rights within the international community; and Chafee's principled opposition to McCarthyism during the second Red Scare. As such, Smith's monograph extends our understanding of Chafee's political and legal philosophy and of how restrictions on First Amendment rights gained popular acceptance during the twentieth century, particularly after 1945. There are, however, major gaps in this narrative.

Smith recounts Chafee's opposition to loyalty oaths but not to federal loyalty and security programs; he recounts Chafee's opposition to the Palmer Raids and the Justice Department's prosecution of radicals during World War I but ignores the scandals of 1923–24 involving the Bureau of Investigation that led to the administrative reforms instituted by Attorney General Harlan Fiske Stone in May 1924; and he recounts Chafee's opposition to the House Committee on Un-American Activities and Senator Joseph McCarthy but does not discuss the revelations about the surveillance activities of the Federal Bureau of Investigation surfacing in the Judith Coplon, Harvey Matusow, and Clinton Jencks cases. Was Chafee indifferent to the latter developments, or did he simply not record his reactions in correspondence or publications? Had Smith supplemented his research into written sources with interviews of Chafee's associates and friends, he might have presented a fuller picture of Chafee's philosophy on the First Amendment and of the impact of his actions.

This is, nonetheless, an important survey of a prominent legal scholar, whose influence derived from his intellect rather than from his political base. By recounting Chafee's intellectual consistency, Smith's thought-provoking survey allows us to understand the change in policy on First Amendment rights during the twentieth century. The contrast between Chafee's consistency and changing policy helps explain why monitoring and containing radical dissent, and the increase in federal surveillance, came to be accepted public policy.

ATHAN THEOHARIS
Marquette University

LAURA KALMAN. *Legal Realism at Yale, 1927–1960.* (*Studies in Legal History.*) Chapel Hill: University of North Carolina Press. 1986. Pp. xii, 314. $35.00.

The history of legal education is rightly an obscure field. Legal philosophy has, for justifiable reasons, frequently been regarded as outside the mainstream of American thought. Even accounting for the paucity of literature about the history of legal education and legal thought, it is still surprising that nothing has been written about the Yale Law School. For all its defects, it was intellectually the most important law school in America during the 1930s and 1940s.

Laura Kalman's book makes an important contribution in remedying this ignorance. Indeed, for all but a handful of readers, she may provide more detail than can easily be digested, yet, for those who have had to earn a living as law professors, the book is a delight. It is one of the few books to address what was meant by legal realism, and it is the first serious study of the Yale Law School.

Books on legal realism have tended to be abstract or to concentrate on individuals. Yale, which, with Columbia, was one of the two centers of legal realism, presented an intriguing panoply of realists led by Underhill Moore, Jerome Frank, Thurman Arnold, William O. Douglas, and others. Kalman looks at their work and at legal realism from the ground up. The result is fascinating.

Kalman also provides a portrait of the Yale Law School in those days. Some of the *gros légumes* of higher education—such as Robert Maynard Hutchins—and an array of Yale presidents flit across the pages. The book also brings to life the various federal judges, not only Arnold and Douglas but also Charles Clark, Tom Swan, and others, who perhaps have not had their due in other studies.

An absence of grand theory marks the book. Some will find this disturbing; for others it will be a relief. Some critics will undoubtedly think that, in addition to being too detailed, the narrative, at times, rises little above gossip. So be it. That may indeed be a legitimate charge. Yet in the gossip surrounding the law school are some marvelous nuggets that tell more about how legal education

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developed and, certainly, more about how realism emerged than has any earlier study.

Of course, for some areas Kalman could have employed more middle-level generalizations, yet this book is essential for anyone interested in the development of American legal thinking in the twentieth century. Kalman's treatment of Harvard and its interaction with Yale is both sensitive and perceptive. Her explanation of the stumbling nature of so much of legal realism adds considerable insight to our understanding. Her observations about the demise of legal realism are more sophisticated than a simple assumption that everyone went to Washington and "turned left." Moreover, her treatment of the postrealist period, and, particularly, the relationship between the realist and the process schools, brings a level of sophistication that has not been reached in any other study.

Kalman writes adequately rather than well. Those interested in these admittedly somewhat obscure fields should not find the study dry. More theoretical studies undoubtedly are needed, but Kalman's book is an important foundation block in the study of the American legal profession in the twentieth century.

ROBERT STEVENS
University of California, Santa Cruz

DONALD F. KETTL. *Leadership at the Fed*. New Haven: Yale University Press. 1986. Pp. xiii, 218. $22.50.

The politics of money is a revered theme among students of U.S. history. Examples abound: the debate over colonial issue of paper money, Jackson's fight to abolish the Second Bank of the United States, Populist outcries for cheap money. Nevertheless, the politics of the Federal Reserve System has been largely ignored, that is, until quite recently when political scientists discovered the Fed as a political, as well as economic, institution.

Donald F. Kettl's book is about the political effectiveness of Federal Reserve leadership with the chair in the principal role, especially since 1934. Its main argument is that the power of the Federal Reserve has depended on the political skill of the chair. The author has traced the evolution of the quality of Fed political leadership by choosing select famous episodes in Fed history to illustrate the political acumen, or the lack thereof, of men such as Marriner Eccles, Thomas McCabe, William McChesney Martin, Arthur Burns, and Paul Volcker. Kettl employs archival records to describe and interpret the relationship of each chair to the president, the Congress, bankers, and other constituencies. In Kettl's view the abilities of each chair to forge a political consensus determined the effectiveness of Federal Reserve policy making. The book suffers from several faults. Although a contribution to our understanding of the political effectiveness of the Federal Reserve, the book leaves much to be desired as a treatise on monetary politics. The number and strength of the Fed's various constituencies are defined only vaguely. The author does not make clear how much power is exerted by the banking and construction industries and how it should be measured. Kettl rightly pays greater attention to the president and the Congress, but their role is minimal except in crises.

The author exaggerates the leading role of the chair in shaping monetary policy. Current knowledge and understanding of how monetary policy is supposed to work exerts a far greater influence. The major conflicts generated by the Fed with the president and the Congress have been over purely technical monetary policy matters, not over the locus of power. The issue has always been the wisdom of a particular monetary policy, not who administers it.

Kettl misinterprets two major episodes of monetary policy by attaching a political significance out of all proportion to its importance. He repeats an untenable view that the Fed relinquished its independence to the Treasury during World War II by acquiescing to the Treasury's desire to establish a pattern of interest rates. To the contrary, the official record discloses that the policy of establishing a pattern of interest rates originated solely within the Federal Reserve System, and the Treasury accepted it only after much argument and considerable delay. Moreover, the author attempts to explain Martin's attachment to the policy of "bills only" (conducting open market operations in U.S. government securities that mature in less than one year) as a new policy aimed at reducing the power of the president of the Federal Reserve Bank of New York. Rather, the issue was mainly a technical question about what determined the term structure of interest rates.

In describing and evaluating Federal Reserve performance during the Great Depression, Kettl relies too much on Milton Friedman's and Anna Schwartz's interpretation of monetary history. The reader is not made aware of how controversial that interpretation is. No mention is made of alternative explanations.

ELMUS WICKER
Indiana University

JAMES A. HODGES. *New Deal Labor Policy and the Southern Cotton Textile Industry, 1933–1941*. Knoxville: University of Tennessee Press. 1986. Pp. xii, 252. $24.95.

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The New Deal did little for three hundred thousand southern cotton textile workers. Section 7(a) of the National Industrial Recovery Act, the Wagner Act, and the Fair Labor Standards Act (FLSA) did not substantially benefit the textile operatives of the southern piedmont. Union drives achieved only momentary surges of membership, and the FLSA's minimum wage provisions provided little for even the most ill-paid of the region's impoverished spinners and weavers.

So concludes James A. Hodges in this well-crafted study, which examines the intricacies of labor relations under the cotton textile code and the disappointing CIO effort to bring unionism to the South. Hodges chronicles the great textile strike of 1934 and the resistance of southern operators to the activities of the National Labor Relations Board (NLRB). He exploits oral histories and copious records of the National Recovery Administration, the NLRB, and unions. Firmly grounded in the secondary literature, the book documents the limitations of the new dispensation in labor relations of the 1930s.

Southern textile workers frequently demonstrated resistance to wage cuts and the stretch out. The mill villages nurtured a distinctive culture that had undergirded bitter strikes dating back to the 1880s. Owing in part to the weakness and ineptitude of the AFL's textile unions, however, workers displayed little sustained enthusiasm for organization. The organizing drives of the 1930s established beachheads of strength, but neither the AFL nor the CIO's more amply funded Textile Workers Organizing Committee could break through generations of skepticism and disappointment. Depending both in 1933–34 and 1937–41 on a vacillating and cumbersome federal presence, neither union campaign moved beyond a small cadre of militants to reach the majority of mill workers. New Deal administrators, well aware that the factory owners held the high cards, largely ignored or sidetracked the unions and local activists. Even the bolder NLRB made little headway.

Hodges's account, although critical of the poor strategy and provincial leadership of the unions, stresses the southern workers' skepticism and fear in accounting for labor's defeats. In so decentralized and economically vulnerable an industry, he argues, the instruments available to unionists and sympathetic governmental officials were simply too blunt to permit the kinds of union growth possible elsewhere.

This well-produced volume is clearly written and contains excellent photographs. Fair-minded and judicious, it reflects sympathy with the textile workers, thorough understanding of the New Deal agencies, and balanced assessment of the activities of the national unions. It will stand as the authoritative account.

ROBERT H. ZIEGER
University of Florida

M. SUE KENDALL. *Rethinking Regionalism: John Steuart Curry and the Kansas Mural Controversy*. (New Directions in American Art, number 2.) Washington, D.C.: Smithsonian Institution. 1986. Pp. 164. Cloth $39.95, paper $19.95.

Between 1937 and 1942 John Steuart Curry (1897–1946) painted murals for the statehouse in Topeka, Kansas, for which a group of newspaper editors paid 70 percent of the cost. In the midst of great publicity Curry painted four walls but did not execute eight panels because the legislature refused to authorize funds. Readers familiar with controversies over murals in the 1930s will appreciate the machinations over the commission, the popular wit of the critics, the dissatisfaction with the uncurled pigs' tails, and the fears that the style would be too modern or that the view of Kansas would be unfavorable.

M. Sue Kendall re-creates the cultural context of these ostensibly historical murals and finds the need for a "usable past" particularly urgent in a state that was noted for drought, dust, and twisters. Curry ignored native Americans and began his history of Kansas with Coronado and Father Padilla, neither of whom may have ever set foot there. Kendall quotes the text on roadside markers and discusses the promotion of "Coronado's Entry" (1541–1941), both part of a campaign to entice the new roadside tourists to the state. Kendall analyzes Curry's famous portrait of John Brown, based on Michelangelo's *Moses*, and reviews Curry's strong antipathy to lynching and his ambivalence about Brown. For Curry, Brown was both an abolitionist hero and a fanatic, whose force, likened to the tornado in the background, could lead to an apocalyptic destruction. Kendall relates these attitudes not only to the approaching war in Europe but also to contemporary revisionist views of the Civil War and to Curry's millenarianism. The author interprets the farm family in accord with contemporary sociological views and the idyllic image of Kansas farming in light of the artist's passion for the agricultural reforms associated with the New Deal.

Although the book ends abruptly rather than with "a rethinking of the phenomenon of Regionalism in American art" (p. 14), the author identifies the contradictions inherent in the demand for regional art in the 1930s. The book sometimes reads like a scrapbook, but the author presents the reader with excellent quotations, interesting sources, and a lively if sometimes repetitive narrative. The book should

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be of interest to historians and art historians, as well as to those in the fields that inspired it—American studies and material culture.

MARLENE PARK
John Jay College of Criminal Justice and Graduate Center
City University of New York

WILLIAM J. BAKER. *Jesse Owens: An American Life*. New York: Free Press of Macmillan. 1986. Pp. xii, 289. $19.95.

The past decade-and-a-half has witnessed the development of the study of sports as a serious branch of social history. The writing of scholarly biographies of athletes has contributed to this process. Biographical studies of Jack Dempsey and Jack Johnson by Randy Roberts and of Babe Ruth by Marshall Smelser, for example, have tried to place sports figures in their wider historical context. William J. Baker has made a solid contribution to this burgeoning field with his fine study of Jesse Owens.

Baker has carefully mined the major primary sources and secondary studies and has conducted exhaustive interviews with Owens's family and colleagues. He deftly interweaves the track star's personal life and professional career; the portrait that emerges is complex, ambiguous, and, finally, positive.

In some ways, Jesse Owens appeared to be a kind of black Horatio Alger. Born in poverty in rural Alabama, he, like many blacks, moved north with his family to Cleveland, Ohio. "Discovered" by a sympathetic white high school coach, Owens was a nationally recognized runner and broad jumper. A track star at Ohio State, he became a national hero with his four gold medals in the 1936 Berlin Olympics. In the words of one journalist, "In what other country would this saga of Jesse Owens come true? An American Negro youth ventures onto the stage occupied prominently by a Nazi dictator and steals the spotlight from him for a little while. The bright glow of romance hovers over such a feat" (p. 4).

Baker covers territory quite familiar to sports historians. His detailed analysis of Owens's life after Berlin is his greatest contribution, for Owens did not "die" after 1936. In spite of a checkered career—whose low point came when he engaged in races against horses a few years after the Berlin triumph—Owens achieved a measure of success. He became an avid proponent of education (perhaps because of his failure to graduate from college), a polished public speaker, and, above all, a promoter of the traditional American values of opportunity and social mobility. Serving as a consultant for both the State Department and corporations such as Atlantic-Richfield, Owens acted as a counterpoise to black activists of the 1960s. In hundreds of public appearances, he condemned radicalism and asserted the essential openness of American society. As Baker concludes, "His youthful optimism undiminished by the years, he voiced an age-old American belief. 'In America anyone can become somebody'" (p. 237).

This is a model sports biography. Marred only by a few instances of sports writers' usage and perhaps a too uncritical view of the role of sports heroes, Baker's book will stand as a major work in recent social history.

ANTHONY O. EDMONDS
Ball State University

PATRICK S. WASHBURN. *A Question of Sedition: The Federal Government's Investigation of the Black Press during World War II*. New York: Oxford University Press. 1986. Pp. ix, 296. $19.95.

This is an account of the federal government's interest in the black press during World War II—an interest reflected, as Patrick S. Washburn clearly shows, in continuous investigation and repeated conferences between government officials and black journalists. The object of official concern were reports and editorials, especially in the early years of the war, that tended to highlight injustices suffered by blacks, particularly those serving in the armed forces. The war offered an opportunity for black journalists to dramatize the hypocrisy of a nation that labeled the war a struggle between the "free" and "slave" worlds and sought to distinguish itself from its racist adversaries yet continued to tolerate and perpetuate various racist outrages. Black journalists insisted that America reconcile its practices with its professions and apparently hoped that sensational exposé would move the nation's authorities in this direction. Black publishers were also aware that newsstand sales, on which their survival depended, rested in large part on the headlining of racial incidents.

Government authorities, especially army officials, thought such reports unjustified and sensationalized. At the very least, they embarrassed the war effort. President Franklin D. Roosevelt and others, noting that Japanese propagandists had targeted the black population, also concluded that black journalism was aiding the enemy by inciting disaffection in the black community and mutiny among blacks in the service. They demanded that the Justice Department determine whether segments of the black press might be charged with sedition.

The department under Francis Biddle refused to bow to such pressure, a stand Washburn attributes to Biddle's dedication to civil libertarian principles. He suggests that the attorney general, almost alone,

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resolutely kept the black press circulating and its editors out of jail. Of course, Biddle's task was made easier by black publishers' tendency later in the war to diminish the kind of coverage officials had found obnoxious. This the author attributes to a number of factors, ranging from the publishers' fear that officials might take action on their numerous threats to an increase in advertising revenues, which made the press less dependent on muckraking for survival.

Washburn is to be commended for illuminating Biddle's role in defense of civil liberties, but he might have made clear that the government by and large secured its repressive purposes. Black journalists were not jailed, but they were quieted, as were a number of other suspected wartime "seditionists," some of whom were imprisoned.

Washburn's clearly written account is based on impressive research, and readers, particularly those concerned with the Afro-American experience, may find it useful. The author, however, has not done justice to the complex civil liberties issues of which the episode he describes was a small part.

RICHARD W. STEELE
San Diego State University

YUR-BOK LEE and WAYNE PATTERSON, editors. *One Hundred Years of Korean-American Relations, 1882–1982*. University: University of Alabama Press. 1986. Pp. x, 188. $24.95.

JOO-HONG NAM. *America's Commitment to South Korea: The First Decade of the Nixon Doctrine*. (LSE Monographs in International Studies.) New York: Cambridge University Press. 1986. Pp. x, 218.

When Fred Harvey Harrington wrote his classic study *God, Mammon, and the Japanese: Dr. Horace N. Allen and Korean-American Relations, 1884–1905* (1944), Korea attracted so little interest that he "decided to leave *Korea* out of the main title" (Lee and Patterson, p. 46). This review assesses two new additions to a swelling stream of literature on a subject that escaped obscurity with the start of the Korean War.

One Hundred Years of Korean-American Relations contains five revised papers from two conferences commemorating the anniversary of the Shufeldt Treaty of 1882. After an introductory essay Yur-Bok Lee's article describes the "one-sidedness" of early relations, featuring "Korea's unfailing but unwarranted faith in the United States and the American government's lukewarm and reluctant participation in the modernization of a remote and tiny kingdom" (pp. 30–31). Harrington criticizes the United States for entering "Korea on a trail of blood" (p. 67) and then sending a "sorry batch" of representatives who tragically encouraged King Kojong's false hope that if he granted them "profitable concessions" this would ensure U.S. "aid in times of crisis" (p. 61). Wi Jo Kang surveys the "stressful and contentious" (p. 69) relations between missionaries and Japanese occupation officials after 1905, and Robert T. Oliver discusses the emergence and survival of the U.S. alliance with South Korea despite "recurrent transitional shocks" (p. 86). Finally, Tae-Hwan Kwak and Wayne Patterson competently analyze how the "Vietnam syndrome" created pressures for a reduced American commitment in Korea until 1984 when "security concerns were no longer an issue" (p. 125).

In the opening essay Patterson and Hilary Conroy identify "duality and dominance" as the "unifying element" in both the book and "this complex century-long relationship" (p. 1). These themes are not consistently developed and the failure to include an article on North Korea constitutes an inexcusable and fatal flaw. Lee's essay is excellent, using Korean and Japanese sources to show how Kojong wanted the United States to be Korea's "new elder brother," providing not just protection "but also a boost to Korean development" (p. 40). By contrast, Kang's superficial contribution never fully explains how missionaries "provided a channel of information and communication for Koreans" (p. 68) and how some promoted Japanese control. But Oliver's article is a disgrace. Almost totally ignoring recent research and writing on Korea, Oliver repeats the threadbare arguments he advanced forty years ago stressing American appeasement and South Korea's role "as a bastion of defense against further Communist advance" (p. 86).

Joo-Hong Nam's book examines "the role of US forces in South Korea and those interests . . . which have pressed for and against a US military presence" (p. ix). Searching for flexibility after the Vietnam debacle, the United States tried to use the Nixon Doctrine "to decouple the military commitment to the country from the politico-moral commitment" (p. 179). Detente with China made military disengagement seem feasible but, Nam contends, created uncertainties about the U.S. commitment to South Korea that increased local instability. Because of his complex and repetitive writing style, the author has difficulty in proving that a credible deterrent required U.S. troops. Constantly using the phrase "in other words," he restates countless confusing points, often making them unintelligible. This may be excusable for an author not writing in his native language, but Nam makes other mistakes, such as referring to John R. Hodge as military governor and dating the creation of the State-War-Navy Coordinating Committee in 1947. Worse still, his documentation is suspect; he cites a State Department memorandum regarding Korea's division when it actually discusses Outer Mongolia.

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Had Nam eliminated the first third of his book and condensed the remainder dealing with the first decade of the Nixon Doctrine, he would have produced a useful essay.

Both books rely on the implicit assumption that the United States has not done enough for Korea in the past and must do more only for the Republic of Korea in the future. If this is the best scholars can say about a century of Korean-American relations, many observers will have yet another reason to regret the Shufeldt Treaty, American rather than certain Soviet occupation of South Korea in 1945, and the U.S. decision to intervene militarily in the Korean War.

JAMES I. MATRAY
New Mexico State University

WILLIAM F. LEVANTROSSER, editor. *Harry S. Truman: The Man from Independence.* (Contributions in Political Science, number 145.) New York: Greenwood, under the auspices of Hofstra University. 1986. Pp. x, 427. $45.00.

In April 1983 Hofstra University held its second annual conference on presidents, with Harry S. Truman as the subject. This volume contains a number of the speeches and papers presented at that conference.

As is invariably true with this type of collection, the papers are uneven in quality. Charles J. Tull leads off with a useful survey of historiography on Truman. The section on China and Israel contains five essays on Truman's foreign policy with regard to those nations, and essentially they retrace old arguments in new ways and add little to our understanding of the problems involved. June Grasso's informative essay on the beginning of the American Two-China Policy is the exception.

The section titled "The Cold War Legacy" breaks new ground in a number of ways. David W. Folts's essay on the development of economic warfare against the Soviet Union, Robert M. Hathaway's paper on Yugoslavia, Leonard Dinnerstein's account of the problem of Displaced Persons, and Darrell B. Montgomery's tracing of the evolution of the postwar nuclear strategy are all well researched and explore little-known aspects of Truman's presidency. The following section contains essays on public opinion regarding Truman and how he handled the news media.

Maurine Beasley contributes a paper on Bess Truman and the press, and Cynthia E. Harrison traces the fight for national legislation for women during Truman's administration. Both essays are informative, although Beasley's, like this volume in general, is marred by typographical errors such as the statement that Bess Truman "rode heard on his [Harry's] use of profanity in public, administering a tongue-lashing when he used the term SOB in referring to columnist Pearson, and when he threatened to beat up a music critic who scorned his daughter's signing" (p. 210).

Donald K. Pickens's paper on Truman's Council of Economic Advisers is valuable. Two other essays on economic policy—Geofrey T. Mills's on price controls and Arthur M. Schaefer's on the reconversion period—are informative, although their conclusions on Truman's success with price controls appear to contradict each other. Both essays are well researched, but the evidence adduced better supports Mills's conclusion that Truman was successful during the Korean War than Schaefer's conclusion that Truman failed.

Editor William F. Levantrosser unsuccessfully tries to explain why an interesting essay by Thomas Heed ("Head" in the list of contributors) on Truman's role as county judge is included in the section on Truman and the courts. Frank Schwartz's "Truman's Seizure of the Steel Mills as an Exercise of Active-Positive Combat" applies James David Barber's thesis to this important constitutional crisis. In "Truman's 'Biggest Mistake,' Tom Clark's Appointment to the Supreme Court," Dennis D. Dorin gives an excellent account of Clark's appointment to, and valuable work on, the Supreme Court.

Two of Truman's appointees, Leon H. Keyserling and Clark M. Clifford, and Margaret Truman Daniel offer reminiscences. Finally, Monte M. Poen argues that Truman's unmailed letters and memoranda "project the image of an honest man" rather than "a bully without courage" as Garry Wills has charged (p. 375).

On balance, this collection makes an important contribution to our knowledge of Truman's administration, and scholars of Truman will certainly want it on their shelves.

R. ALTON LEE
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CARL M. BRAUER. *Presidential Transitions: Eisenhower through Reagan.* New York: Oxford University Press. 1986. Pp. xvii, 310. $22.95.

Carl M. Brauer's study, commissioned by the Kennedy School of Government at Harvard, is a worthy successor to Laurin L. Henry's *Presidential Transitions* (1960). Along with Henry, Brauer extends the view of what constitutes a transition well beyond the formal inauguration of each new president. Despite its title, therefore, this study transcends the period between election day and January 20 and offers well-considered evaluations of how John F. Kennedy, Richard M. Nixon, James T.

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Carter, and Ronald Reagan coped with the problem of establishing effective authority.

The broader view of the transition period makes much sense. Ending the Korean War certainly was the fruition of Eisenhower's victory over Truman, and so analyzing the steps that led to the truce constitutes a logical completion of that phase. The agreement at Panmunjom gave Eisenhower the greatest "transition accomplishment" of any president (p. 60). That Nixon, also contending with a war the electorate expected him to end, failed to accomplish his mission until his second term, suggests the naiveté with which he assumed power in the first place, an attitude imbued with "innocence and hopefulness," as well as his own "particular self-image as a tough, determined, and destined world leader" (p. 169). Carter, on the other hand, was unclear about his goals. Whereas vagueness was a useful ploy for others, in his case it inhibited any sense of leadership. If, like Nixon, he was innocent about the realities of power, his weakness came from failing to clarify his priorities, never getting beyond the ambiguity of his inaugural address. That his transition funding was far greater than what was available to earlier presidents-elect was irrelevant.

There is no simple formula. Successful transitions are more likely to reflect the strengths and weaknesses of the individual than the quality of planning. That became abundantly clear in the case of Reagan, who declared he had won a "mandate," then moved single-mindedly with an ideologically compatible crew, charming, and proselytizing all the way. Moreover, as Brauer shows, each incoming president has seized on the assumptions about his predecessor that best served his preconceived notions. They, too, read history to justify what they thought should be done.

Although the transition may determine the quality of much to come, a weak one does not rule out future recovery. One must then point out that the reverse may also be true, that the glow of early success may be insufficient to prevent later disaster. It may even contribute to that self-destructive hubris. Except for the unusual circumstances of Kennedy and Gerald Ford, whose interim presidency is not discussed by Brauer, the most divisive years for each postwar presidency have come at the end. That thought is sufficiently sobering to warn against attempting, as does Brauer in a strong, confident chapter, to analyze the relationship between transition and performance in a current administration. He is on firmer ground when warning that "the importance of transitions should not be exaggerated" (p. 268).

HERBERT S. PARMET
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WAYNE HAMPTON. *Guerilla Minstrels: John Lennon, Joe Hill, Woody Guthrie, Bob Dylan*. Knoxville: University of Tennessee Press. 1986. Pp. xiv, 306. $24.95.

This book focuses on the culture of protest songs and heroism's role in that tradition. Wayne Hampton centers on four key folk singers who personify his concept of the "guerrilla minstrel." Most would agree that John Lennon, Joe Hill, Woody Guthrie, and Bob Dylan are influential examples of America's protest song tradition. A few (myself included) would argue that Phil Ochs was an even better example of that tradition.

The problem here, however, is not the choice of singers but the way they are analyzed. After an opening chapter on the working-class hero—a concept attached to all four singers because of their social consciousness, not their social origins—Hampton focuses first on Lennon. Then, following a chapter on "utopian sensibility" and protest singers, the author treats Hill, Guthrie, and Dylan in separate chapters. A concluding chapter gives final impressions of the four subjects as guerrilla minstrels.

I found the last chapter the most rewarding. Here, Hampton makes his points in a useful historical context and carries on an impressive discussion of heroic image. Earlier on, the book suffers from a warped chronology. Because he treats Lennon before the prototype singers, Guthrie and Hill, Hampton's discussion of the protest tradition that shaped Lennon becomes anticlimactic. The most original commentary covers Lennon, who has seldom been seriously discussed outside the context of pop music. The analyses of Hill, Guthrie, and Dylan do not generally break new ground. Thus, after a strong start with Lennon the book runs downhill, until it is salvaged by a useful final chapter that develops a working context.

Hampton strains to bring his diverse subjects under a common theoretical framework. In the process, the prose sometimes lapses into hazy jargon. For example, we learn that Dylan's "transcendental mysticism replaced the ideology of collective action" (p. 214). Despite the problems, Hampton's theoretical approach to protest music deserves to be taken seriously. Writers seldom agree on what cult figures such as Lennon and Dylan represented. To rigidly define them as "guerrilla minstrels" is as futile as categorizing Abraham Lincoln only as a frontier politician. Yet, overall, the book is suggestive, and a very good, detailed discography makes it even more useful for future researchers.

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LARRY E. CABLE. *Conflicts of Myths: The Development of American Counterinsurgency Doctrine and the Vietnam War*. New York: New York University Press. 1986. Pp. xiii, 307. $30.00.

Larry E. Cable's thesis in this book is simple: the American obsession with the external role of North Vietnam in the insurgency in South Vietnam drove the war for Southeast Asia into a major conventional conflict. The conventional war could not be won by the United States and the South Vietnamese government. "One of the greatest ironies of the Vietnam War was that the Americans, in an attempt to solve a problem that did not exist, created a problem that could not be solved" (p. 225). The American military escalation, designed to halt or at least slow Communist infiltration, set off a conventional invasion by the People's Army of Vietnam. "The American escalation, far from stopping the North, served to stimulate an equal and opposite Northern escalation. Newton had been read by Ho" (p. 246).

Cable's thesis is hardly novel—and he does not claim it is—but it has a novel twist, which is that the American military believed that "people's wars" had been defeated by strategic interdiction. The anticommunist forces won in Greece, South Korea, the Philippines, and Malaya because they isolated the guerrillas from external support—or so Americans thought. Even though internal reforms swung the balance, American military doctrine became skewed to "barrier operations," not the control and pacification of the populace. Only the Marine Corps, bloodied in interwar counterinsurgency campaigns in Haiti, the Dominican Republic, and Nicaragua, produced a doctrinal statement (the *Small Wars Manual*) that stressed the internal causes and conditions of popular revolt.

Despite its thinness in research, awkwardness in style, and passion in argument, *Conflict of Myths* is a serious book on a significant issue. Did American policy makers misunderstand the challenge in South Vietnam? Did the armed forces try to reshape the war into one they could fight more effectively, if more expensively? Cable's evidence brings the Scots' verdict: "not proven." Whatever Cable reads in army internal documents and National Security Council studies he does not close the gap by examining the role of individual military officers or the Joint Chiefs of Staff in the decision-making process for Vietnam. One should still look to the earlier books by Leslie Gelb, Richard Betts, and Douglas Blaufarb for such edification. The evidence still suggest that the earlier military experiences, scantily captured in military instruction in the early 1960s, had little influence on counterinsurgency planning in Vietnam.

A more serious flaw of the book is its failure to deal with whether policy makers truly exaggerated Communist infiltration. Cable certainly thinks so, but the intelligence assessments then and scholarship now (especially by experts such as Douglas Pike) suggest that the policy makers had a fair view of the problem. They also saw all the flaws of the South Vietnamese government and armed forces, both before and after the fall of Ngo Dinh Diem. Lyndon B. Johnson's administration, however, saw no option that offered a rapid resolution of the war independent of the performance of the South Vietnamese government except, perhaps, by limited strategic interdiction. Cable argues that the "combination of ideology and unquestioning acceptance of the Clausewitzian priority upon destruction" (p. 285) turned the war into an unwinnable conventional war, but he does not address why the war, after 1965, was unwinnable. Further consideration of the relationship of political ends and military means might have clarified Cable's central intellectual and emotional problems: he cannot decide why the United States should have been fighting (no matter how) for South Vietnam at all. Historical experiences in other parts of the world cannot provide a complete answer to that issue.

Cable's book, whatever its flaws, asks important questions about the American commitment to Southeast Asia, and it provides a forceful, stimulating discussion of the conceivable relationships between military operational doctrine and geopolitical decision making. Whether or not one agrees with Cable's analysis, his effort is laudable.

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CANADA

DOUG OWRAM. *The Government Generation: Canadian Intellectuals and the State, 1900–1945*. Buffalo, N.Y.: University of Toronto Press. 1986. Pp. xiv, 402. Cloth $45.00, paper $19.95.

What social forces and intellectual currents explain the changing role of the Canadian state in the early twentieth century? In creative and stimulating ways, a number of Canadian historians have tackled this question in recent years, and the present book attempts both to synthesize and build on this work.

According to Doug Owram, by the mid-1930s, an "intellectual elite" committed to Keynesian economics, efficient planning, and the welfare state had moved into the "inner circles" of the federal government. Over the next decade this group modernized the policy and operations of Canadian government. The demands of an industrial society, the growth of professional training, and the tensions of Canadian federalism explain both the conditions and limita-

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tions within which the new "intelligentsia" emerged and functioned.

The book succeeds best in discussing how World War I and the growth of social science in Canadian universities affected the thinking of some influential Canadian intellectuals. Increasingly, they viewed the legitimate role of the state pragmatically, not idealistically, and those among them who later attained influence were able to practice what they preached.

But the author leaves unresolved a number of important, puzzling questions. Who merited membership in the new intelligentsia? One never finds a single list of such individuals, but they appear to include everyone, both in and out of government, who believed in the "positive" and efficient state. This approach blurs important ideological and political distinctions between conservatives, liberals, socialists, and communists and understates the importance of these differences in Canadian politics in the 1930s and 1940s. Individuals such as Stephen Leacock, Frank Underhill, and Mackenzie King are included in the intelligentsia when they spout the statist, technocratic rhetoric of civil-service mandarins O. D. Skelton and Brooke Claxton but are otherwise marginalized when their ideas drift to the Left or Right.

The author appears to exaggerate the role of the new elite. If it was so influential then why did it accomplish so little? King was prime minister for twenty-two years, but for most of that time he was unmoved by the progressive spirit despite being surrounded by "reformers." Owram does not adequately explain this paradox. In fact only two important federal acts instituted in the early 1940s survived in peacetime: Unemployment Insurance and Family Allowances. But, as the author notes, Unemployment Insurance involved little state intervention and spending because it was instituted at a time of full employment, and Family Allowances had never been a crucial demand of reformers—the measure was conceived and implemented in an ad-hoc fashion in the months leading up to the federal election of 1945. Indeed, the end of the war witnessed a massive retreat from efficient state planning, as wage and price controls were removed, and productive, profitable crown corporations were turned back to the private sector. Owram's main characters are strangely silent on these actions.

Twenty years elapsed before the welfare state fully emerged in Canada, which seems to diminish the long-term intellectual and political significance of the pre-1945 intelligentsia. In sum, Owram's detailed study raises interesting questions about the nature of policy making, retraces some familiar terrain, and ends a generation too soon.

PAUL AXELROD
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MILDA DANYS. *DP: Lithuanian Immigration to Canada after the Second World War.* (Studies in Ethnic and Immigration History.) Toronto: Multicultural History Society of Ontario. 1986. Pp. 365. Cloth $19.95, paper $14.95.

The years after World War II witnessed a dramatic change in Canada's refugee policy. In keeping with its new commitments to the United Nations and to human rights in general, Canadians welcomed over one hundred fifty thousand European refugees and Displaced Persons (DP) between 1946 and 1952.

Milda Danys focuses on the experience of one DP group, Lithuanian refugees, but her book is by no means a narrow and ethnocentric account. In the sections devoted to their European background, she compares the plight of the Lithuanians to millions of other people caught between Nazi and Soviet terror. These traumatic years are skillfully analyzed and made more poignant through the recollections of scores of Lithuanian refugees, many of whom were youngsters at the time. Sanctuary in Canada was one form of escape from the Displaced Persons camps, and about ten thousand Lithuanians took advantage of the opportunity. But, as Danys demonstrates, adjustment to Canadian society was not always easy. First, refugees needed to pass the critical examination of Canadian medical and occupational selection teams. Lithuanian DPs learned quickly the importance of appearing willing to accept grueling jobs in sugar beet fields, lumber camps, mines, and, for Lithuanian women, Canadian middle-class homes. In the best section of the book, Danys describes and dissects the various contract-labor schemes for immigrants. Danys examines both the perspective of the bureaucrats in Ottawa and the viewpoint of the Lithuanian DPs themselves. Drawing on over two hundred interviews, she brilliantly depicts the personal side of this standard two-year period of indentured labor.

For most Lithuanian DPs their Canadian odyssey meant downward social mobility. Many were professionals, but they came as low-status workers, often ridiculed because of their poor English. As one immigrant stated: "It wasn't Canada's fault that I was so unhappy. But I wasn't prepared for that kind of work, to live like that" (p. 25). Yet, according to Danys, most of these people were characterized by an intense desire to succeed in their new environment. "If the only road to an independent life lay through the tunnel of (a) gold(mine), then that was the way the DPs would go" (p. 115). Fortunately, their sense of collective identity provided vital psychological and social support, and "the tragic rupture of their lives only heightened their belief in the family, the country, work, education and progress" (p. 126).

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The blending of personal experience and general commentary is both a strength and a weakness of this book. Did, for instance, Ukrainian, Jewish, Yugoslav, and Mennonite DPs demonstrate the same frustration with agricultural or domestic service jobs? Did members of these groups, as did the Lithuanians, show a propensity to identify with their homeland and to regard themselves more as émigrés than immigrants? Such basic questions are not answered here. Nor does the book offer much information about the general reaction of Canadians to the DPs. Neglected also are the many fruitful comparisons that can be made with the experience of postwar DPs in the United States.

These criticisms aside, this is a stimulating, well-written, and informative account of one of the most unique groups of immigrants to have sought a better life in the Dominion of Canada.

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LATIN AMERICA

HARRY KELSEY. *Juan Rodríguez Cabrillo*. San Marino, Calif.: Huntington Library. 1986. Pp. 261. $25.00.

In 1901 an archaeologist doing research on Santa Rosa Island off the coast of Southern California accidentally discovered a stone metate with some curious marks on it. Although he was unable to decipher them, the stone eventually was deposited in the museum collection of the University of California at Berkeley, where it remained forgotten for many years. In 1973 it was concluded that the marks represented the initials "JRC" and that the stone had once covered the grave of Juan Rodríguez Cabrillo, who led the first European expedition to the shores of Upper California in 1542. Harry Kelsey has written a narrative account of the life and career of Cabrillo based mainly on information derived from unpublished documents in the archives of Spain and Guatemala. One of the principal strengths of this volume is the use of original sources to reconstruct Cabrillo's biography.

The author traces Cabrillo's career from his early years in Seville through his participation in the conquest of Mexico, where he served as a captain of crossbowmen. After the Spanish retreat from Mexico City on the *noche triste*, Cabrillo was placed in charge of preparing the ships to be used in the final assault on the Aztec capital. With the conclusion of the conquest of Mexico, he accompanied Pedro de Alvarado to Guatemala, where he obtained wealth and influence. The expedition to California in 1542 marked the culmination of his career and his last undertaking, for he died on this voyage in 1543. The California expedition was a major enterprise, not just a reconnaissance of the coast. The objective was to seek a northwesterly route to Asia since it was assumed that North America was either an extension of the Asian mainland or very close to it and that the Moluccas were islands near this shared coastline.

The strongest chapters in this book deal with the planning and realization of the California voyage and the controversial authorship of the account of the trip. Another important chapter discusses the historiography of this expedition. Less successful is the author's attempt to clarify Cabrillo's origins and place of birth. Certain factors relating to the explorer, such as the Cabrillo name and his reluctance to use it for most of his life, suggest a possible *converso* origin. A large bibliography exists on the *conversos* that could have been consulted by the author. This material, in conjunction with the extant genealogical information for Cabrillo's descendants, could perhaps have helped solve the mystery of Cabrillo's origin and would have further enriched this interesting and valuable study of an almost forgotten Spanish explorer and conqueror.

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LOUISA SCHELL HOBERMAN and SUSAN MIGDEN SOCOLOW, editors. *Cities and Society in Colonial Latin America*. Albuquerque: University of New Mexico Press. 1986. Pp. xi, 350. Cloth $30.00, paper $14.95.

This compilation of high-quality essays on urban social history in colonial Latin America synthesizes much of the literature that has emerged on this topic over the past twenty-five years. The essays are original contributions written for this collection. Each concentrates on a single urban social or occupational group, namely, *hacendados*, merchants, bureaucrats, churchmen, female religious, the military, artisans, and the underclass, as well as suppliers, sellers, servants, and slaves, who are treated as a unit. Louisa Schell Hoberman and Susan Migden Socolow, who offer an unusually useful and probing introduction and conclusion, and all of the contributors have previously written on their topics in more specialized monographs, articles, and theses. In this book they speak more broadly about the composition and activities of these groups throughout Latin America and across the entire colonial period. The result of this approach is a set of solid surveys, including several masterful statements of our present knowledge about one or another group. Especially distinguished are Catherine Lugar's piece on merchants, Mark Burkholder's on bureaucrats, Paul Ganster's on churchmen, and

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Mary Karasch's on suppliers, sellers, servants, and slaves. The editors' efforts to have the authors address similar issues are notably successful and give the collection a unity and an evenness in tone and coverage rarely found in this type of book. The distinctive qualities of each group are nonetheless denoted.

The authors treat all the major cities, including those of Brazil, throughout the colonial period. The contributors tend, however, to emphasize major cities, usually capitals, such as Mexico City, Lima, and Salvador, rather than sizable provincial centers, mining towns, and late emerging cities such as Buenos Aires, Guadalajara, and Caracas. Further, the essays reflect the emphasis of much of the pertinent historical literature on the late colonial period.

Some specific shortcomings appear. Susan Ramirez's essay on *hacendados* lacks the scope and coverage attained by the others. It draws too heavily from her own research on northern Peru instead of discussing the many publications on this group in any representative way. The role of market forces and the impact of urban development on the formation and character of rural estates are not treated. Similarly ignored are the process by which urban dwellers became estate owners and the broader issue of interconnections between the city and the countryside. Asunción Lavrin's essay is quite fine, but it restricts itself to female religious instead of covering all urban women. Her section, valuable as it is, devotes as much discussion to convents as functional units in the society and the economy as to the social background and behavior of the nuns. Christon Archer's contribution on the military is likewise a useful statement that draws on his unsurpassed understanding of that institution in the colonies. But the military as an entity did not appear in the central colonial areas until well into the second half of the eighteenth century. Even then, only a small number of officers and soldiers were full-time professionals; the other men constituted a militia force drawn, when necessary, from the other social groups treated in the book. Karasch's chapter on suppliers, sellers, servants, and slaves focuses primarily on Brazil and does not much differentiate between slaves and free blacks or systematically discuss manumission. Gabriel Haslip-Viera, in his treatment of the underclass, is compelled by the nature of the literature to speak at length about squalor, criminality, and attitudes toward the group, rather than about its internal differentiation and values.

As a group, the essays are sensitive to such issues as career patterns, the centrality of family to social position and behavior, the diverse functions of marriage, linkages among regions and groups, and the process of racial mixture. This book is intended to serve as a set of readings in college courses and would fill that function admirably. The authors contribute annotated bibliographies at the end of their chapters, and the book contains an excellent glossary. This volume represents an important distillation of the many valuable but diffuse writings on colonial Latin American urban societies and bespeaks the growing maturity of the field. Any Latin Americanist, whether primarily a colonialist or not, would benefit from reading this fine collection.

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Washington State University

BRIAN R. HAMNETT. *Roots of Insurgency: Mexican Regions, 1750–1824*. (Cambridge Latin American Studies, number 59.) New York: Cambridge University Press. 1986. Pp. ix, 276.

This new book by Brian R. Hamnett proposes a profoundly new set of perspectives on the Mexican War of Independence (1810–21). It raises more questions than it answers and thereby provides a challenging agenda for further research by specialists in the late colonial, independence, or early national fields. Purposely speculative, the book was not meant to satisfy its readers but to provoke them. Viewing the phase led by Miguel Hidalgo as an insurrection, and everything that came after it as an insurgency, Hamnett questions whether the Mexican War of Independence was anything like the national crusade that it is always portrayed as having been. Was it, rather, the product of myriad local and regional social and economic tensions that broke out at a moment of imperial confusion and viceregal weakness? What really was the impact of the famine of 1808–10? Why, besides Hidalgo and José María Morelos, did not identifiable "national" leaders of the movement emerge until the rise of Agustín de Iturbide? Was Morelos even a truly national figure? Why was there no consistent political agenda or program for independence? Who were the "rebels" anyway?

Focusing primarily on the regions of Puebla, Guadalajara, Michoacán, and Guanajuato, Hamnett sketches the deterioration of living standards for the lower class that combined with the complaints of the upper class in the period before 1810. He finds ideological, economic, and racial tensions that existed before, during, and after the War of Independence, which suggests, among other things, the need for a revised periodization. This sort of revision has already been done for the Mexican revolution of 1910, and Hamnett's proposal that it be attempted for the independence era is clearly appropriate.

Studying various local leaders and local issues, Hamnett finds that many of the rebels were little

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more than bandits (as the Spanish royalists always claimed they were). The viceregal government fought the insurgency by using techniques of counterinsurgency that, although spontaneously generated in the rush of crisis, appear quite modern in method. Regional insurgency was often directed at local merchant-investors, fighting was often fierce, and people changed sides with alarming alacrity. Often, local caciques joined the fighting, setting the stage for regional caudillos who would prevail in whole sections of the country after independence. No one controlled the regional rebels; the viceroys could not control the counterinsurgency; and, in the end, rule of force was created that lasted long after independence. The insurgency, spread through a network of contacts among priests, muleteers, and bandits, was amorphous and impossible to pin down. Hamnett's data on regional tensions and insurgent leaders are drawn from a wide variety of local and municipal archives, and general comparisons to similar topics from European historiography frequently inform the discussion.

The overall argument is that recession, famine, dislocation, and momentary governmental loss of control combined in 1810 to convert regional tensions into a genuine insurrection. After 1811, however, with the defeat of Hidalgo, the movement degenerated into local insurrections in which clear cause-and-effect relationships to explain why some men became insurgents and others became counterinsurgents are impossible to find. To Hamnett no "war of independence" occurred, just a series of regionally based disputes on which historians later superimposed nationalism. This is a radically new interpretation of the crisis of 1810–21.

Because definitive proof of these theses would require a multivolume, state-by-state survey, Hamnett is not able to drive the nail in conclusively. The reader is left with questions, ambivalence, and a realization that the whole argument is phrased in terms of possible explanations. The book therefore should provoke controversy. Among many other things, it fails to account for the final achievement of independence in the Iturbide consensus, nor does it adequately deal with the Congress of Chilpancingo and the Constitution of 1813. Several chapters bog down into narratives of troop movements and local rebellions, not always well linked to the broader arguments. The author also presupposes a good deal of knowledge from his readers, so the book will not be of use to the uninitiated. The complaints aside, we can only hope that a book that questions as many sacred cows as this one will receive the wide notice and debate it deserves. Given the new perspectives suggested by Hamnett, the historiography on Mexican independence may never be the same again.

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THOMAS BENJAMIN and WILLIAM MCNELLIE, editors. *Other Mexicos: Essays on Regional Mexican History, 1876–1911*. Albuquerque: University of New Mexico Press. 1984. Pp. xv, 319. Cloth $24.95, paper $12.95.

Over a decade ago historians hastily abandoned their focus on Mexico's center to dig in the foundations of regional history as a means of explaining the genesis and process of the revolution of 1910. Especially stimulated by Friedrich Katz's studies of northern Mexico and Luis González's microhistory of San Jose de Gracia, a village in the west-central state of Michoacan, students of the Porfiriato (1876–1910) sought to define Mexico's regions.

This set of essays by and large follows the division made by Katz: the aggressively capitalist and economically diversified north, the plantation economy of the south dependent on debt peonage, and the center, where traditional agrarian communities conflicted with expanding haciendas in a period of rapid market growth and urbanization. Thomas Benjamin and William McNellie have divided their volume into sections on economic, political, and social history. The essays, most of them well known to students of Mexico and all written by competent scholars, each represent a significant contribution to regional studies: Evelyn Hu-Dehart's analysis of the repression of the Yaqui Indians and rise of the Chinese petty merchants in Sonora; studies of Chihuahuan, Coahuilan, and Puebla politics by Mark Wasserman, William Stanley Langston, and David La France; Frans J. Schryer's originally pathbreaking work on Pisaflores, in which he established the critical role of *rancheros* in the revolution; Allen Wells's description of labor conditions on Yucatán plantations in which he shows that workers were not as quiescent as we had thought; and Daniela Spenser's portrait of the German-dominated coffee economy in Chiapas. The most striking essay, because it has not been previously published and because it focuses on revolutionary organization from the bottom up, is William K. Meyers's account of changing labor conditions and political mobilization in the rich Laguna region of Durango and Coahuila. He shows that proletarianization fostered a concern for wages and working conditions among workers but expressed itself politically in the first moments of revolution in a resurgence of traditional social banditry.

The essays display a general weakness in Porfirian studies: the predominance of a teleological ap-

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proach. Historians examine the Porfiriato to understand the genesis of revolution but seldom to grasp the intrinsic complexities of the period. In this sense, William H. Beezley's historiographical essay is helpful, especially in suggestions for research in social history, the most neglected area of the Porfiriato. He occasionally skews his priorities. At the end of his essay, after suggesting the need for a study of the post office, he recommends that we look at the developing school system. The two problems are related, although not necessarily in that order nor as marginal issues. Recent work makes clear that a notable expansion of the culture of the written word occurred in the period. Whether the actors are François Guerra's modernizing teachers or Alan Knight's *maestros de aldea*, they contributed mightily to a revolution that relied significantly on the written word and championed the further development of schools.

These lacunae notwithstanding, Benjamin and McNellie have put together a solid volume of essays that are especially useful to beginning students of Mexican history at the graduate and undergraduate levels.

MARY KAY VAUGHN
University of Illinois, Chicago

GILBERT M. JOSEPH. *Rediscovering the Past at Mexico's Periphery: Essays on the History of Modern Yucatan.* University: University of Alabama Press. 1986. Pp. xvii, 203. $28.95.

The study of Mexican history has been greatly enriched over the past twenty years by scholars who have narrowed their geographical focus and broadened their methodological and theoretical approaches. One of the most skillful practitioners and eloquent advocates of regional and multidisciplinary history, Gilbert M. Joseph, has taken measure of this decentralization of Mexican history through its transformation of our understanding of modern Yucatán. His essays in this volume offer not only an excellent critique of recent historical (and social scientific) writing on the region but also a useful synthesis of the socioeconomic history of Yucatán from the Caste War to the revolution.

In part 1, "Writing History in Yucatán," Joseph introduces the recent (mid-1970s to the present) historiographical renaissance of Yucatán. A dramatic increase in the number of scholars writing about Yucatán's history has been matched by increased institutional support in Yucatán, Mexico, and the United States for Yucatecan history, the establishment of new journals and publishing outlets, and the modernization of regional archives and libraries. The result, as Joseph signals in his title, is truly a rediscovery of the past. In contrast to the long-dominant tendency of writing either strictly political history of creole elites or ahistorical anthropology of modern Mayan villages, the new history of Yucatán has benefited from a cross-fertilization of history and anthropology to document and explain interconnections between elite Yucatecos and Mayan villagers, between political conflict and political economy, and between Yucatán and Mexico, the United States, and the rest of the world.

In part 2, "From Caste War to Class War: A Survey of Recent Historical Writing on Modern Yucatán," Joseph demonstrates how recent painstaking research has radically revised the history of three periods of the Yucatecan past. The chapter on the period from 1750 to 1880 analyzes the new research on the development of commercial agriculture, the causal relationship between agricultural expansion and the Caste War of 1847, and the consequences of the Caste War on creole capitalist agriculture. The chapter on the period from 1880 to 1915 considers the rash of studies on the political economy of henequen, the impact of the henequen plantation on Indian villages and labor conditions, and the role of North American capital (the International Harvester Company in particular) on the regional economy. The chapter on the period from 1915 to 1940 discusses the work of revisionists on the Mexican revolution in Yucatán.

Throughout, Joseph underscores that recent research has concentrated on uncovering the fundamental patterns of socioeconomic relationships and has ignored the traditional local historiographic themes of political conflict and relations with Mexico. Have local historians contributed so little to an understanding of their society? Remarkably, in the recent boom the agenda of regional historiography has been transformed by a few North American social scientists, presumably writing in behalf of an exploited people without (academic) history and defying the elitist, racist, imprecise, and superficial concerns of creole historians. The "new history" written by dedicated and talented men and women is better history, but what is the significance of such a historiographical revolution from without and above? How will future Yucatecan historians look on this monument to North American social science and academic history?

THOMAS BENJAMIN
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ELIZABETH ANNE KUZNESOF. *Household Economy and Urban Development: São Paulo, 1765 to 1836.* (Dellplain Latin American Studies, number 18.) Boulder, Colo.: Westview. 1986. Pp. xvii, 216. $27.50.

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In her research on São Paulo Elizabeth Anne Kuznesof has sought to link the effects of commercialization—"development" or "modernization," if you will—on households, families, and "individual decisions concerning basic life choices" (p. 2). The present monograph is now the most complete statement of her thinking. It presents her evidence in more detail and restates arguments previously published in scholarly journals. Although this book is more densely textured than anything we have seen before, it is also less elegant and more ponderous than earlier essays.

São Paulo changed rapidly from 1765 to 1836, and Kuznesof closely analyzes those changes with evidence drawn from manuscript censuses and land registers. She presents three "moments," 1765, 1802, and 1836, to characterize "stages" in the political economy of São Paulo, respectively, subsistence, market, and export. In 1765 virtually all households of São Paulo, in "urban" as well as outlying *bairros*, were engaged in subsistence agriculture. By 1802 "urban" can be distinguished from "rural" as agriculture—subsistence and for market—moved from city *bairros* to outlying ones. As specialization set in, the city became dependent on the market for food and other goods. By 1836 agriculture was further commercialized as the transportation network, especially the road to the port of Santos, linked producers of the São Paulo region to world markets.

Although this may sound overly schematic, Kuznesof has ample evidence to justify plotting the city's evolution in this way. Indeed, because change was rapid and the "stages" well defined, São Paulo is an especially useful case for comparison. Moreover, the objective of this study is not to fine-tune nuances of change and continuity but to link broad trends with adaptations to them by households and families. In fact, this linkage is implicit in the censuses, for during the seventy years under study the household was equally the unit of enumeration and of production. Here, the picture becomes nuanced as Kuznesof shows how and why households varied in size and composition, tending to move from nuclear to nonkindred with the onset of commercialization. Kuznesof carefully analyzes shifts in the proportion of households headed by women (29, 45, and 39 percent for the three "moments"), birth rates (which increased until the early nineteenth century and then declined), child abandonment (perhaps two-thirds of all births in the early nineteenth century), sex ratios, occupational structure, and much more. Her findings provide some surprises and much to ponder.

An aspect of *paulista* society that requires further clarification is the place of kinship networks. Kuznesof has established that the composition of households most closely reflected imperatives of production and material life. Yet she also accepts that "kinship is organized as importantly *among* households as within them" (p. 182). Thus, she recognizes that a household-by-household analysis does not identify dispersed patterns of kinship linkages. For the Hispanic world, a strong case for such patterns exists in the literature, and one wonders why São Paulo would be an exception. Kuznesof has unearthed and assembled a rich body of data and has made it yield the kinds of connections that are seldom made. Students of urbanization, development, and the Hispanic world will all profit by a close reading of her book.

RICHARD BOYER
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JOÃO JOSÉ REIS. *Rebellião Escrava no Brasil: A História do Levante dos Malês, 1835* [Slave Rebellion in Brazil: A History of the Muslim Uprising, 1835]. São Paulo: Brasiliense. 1986. Pp. 293.

The African revolt of 1835 in Bahia, also known as the "Muslim Uprising," has long fascinated students of Brazilian history. Although the revolt lasted only a few hours and failed to spread beyond a few neighborhoods in the city of Salvador, its markedly African character and the prominent role played by Muslim slaves and freedpersons in the rebel leadership make it one of the most distinctive slave rebellions in the New World.

The Bahian historian João José Reis has written a remarkably thorough and stimulating study of the revolt of 1835. Given Reis's exhaustive research and his sophisticated analysis of archival materials, his book will probably be regarded as the definitive study of the uprising. One of the most admirable aspects of this work is Reis's skillful analysis of the rebels' testimony, which provides a major portion of his data. Aware of the potential pitfalls in using statements made to the courts by defendants who were both protecting themselves and shielding fellow conspirators, the author carefully distinguishes between statements that were likely to be accurate and those that cannot be considered reliable. For example, the prisoners knew that any association with Islam could seal their guilt in the eyes of the authorities and repeatedly disclaimed any knowledge of that religion. Rather than accept these routine denials at face value, Reis cites testimony by neighbors and evidence unearthed by police searches to show that a substantial number of rebels had some involvement with Islam. Reis's discussion of the role of Islam is extremely interesting. He flatly rejects the notion that the revolt was to be a "jihad"—a Muslim holy war to exterminate or enslave all infidels. He acknowledges, however, the central role played by Islam in binding the conspir-

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ators together and giving them a distinct identity as well as a set of beliefs that transcended antipathies between Haussa and Yoruba, freedperson and slave.

Although the revolt of 1835 forms the centerpiece of Reis's study, this book does much more than examine a single slave revolt. In the course of explaining the causes and context of the uprising, Reis provides a fascinating social history of urban life and the African community in a city that was (and is) one of the most important centers of African culture in the Americas. The section entitled "The African Community in Revolt" will probably be of greatest interest to students of slave societies. Using rebel testimony, police reports, travelers' accounts, and letters of manumission, Reis reconstructs the ethnic ties, family structures, occupational and residential patterns, and cultural activities that characterized Salvador's African community. Here Reis makes a convincing case for the importance of ethnicity and shows that the bond between slaves and freedpersons of African birth was often stronger than that between African- and Brazilian-born slaves. Indeed, for Reis the great divide in Bahian urban society in 1835 was between Africans and Brazilians, the latter including "creole" slaves who regarded the Africans as foreigners and who were much more likely to pursue individual strategies for manumission than collective rebellion.

This book is a model of careful research and skillful argumentation. The data it presents and interpretations it offers should be of interest not only to Brazilianists but also to all historians of slavery and African culture in the Americas. Unfortunately, it is currently available only in Portuguese; let us hope that some English-language publishing house sees fit to make it more widely accessible to historians in the United States.

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WILLIAM F. SATER. *Chile and the War of the Pacific.* Lincoln: University of Nebraska Press. 1986. Pp. 343.

William F. Sater has written a revisionist account of the conduct and consequences of the War of the Pacific. He states that Chile, rather than being a racially homogeneous, well-governed, and cohesive nation that deployed a highly motivated civilian army, was in reality ill-prepared, divided, and unable to organize militarily or financially. The common people did not understand or support the war. Later generations, however, looked back on the war as their heritage of glory and accepted the myths that emerged as truth.

The author discusses the causes and conduct of the war in the first two chapters. The remaining eight cover the conflict between civilians and the military, political infighting, finances, the home front, problems of peace negotiations, and economic consequences of the war.

A large part of the work is concerned with the reluctance of the government to enter the war and the precarious nature of the victory. The author devotes considerable attention to disproving the myths that arose after the war and concludes that the short-term consequences of the war on Chilean economy, society, and government were minimal.

The work is an interesting and valuable discussion of the difficulties the Chilean government faced in conducting and financing the war and in persuading parliament and the people to support it. Ample documentation supports the author's statements, including forty-five tables of statistics in the appendix, covering production, prices, imports, crime, money in circulation, and other matters.

Although the text does not extend beyond the end of the war in 1884, Sater lists the long-term consequences of the war in the epilogue. He states that the war altered the relationship of Chile with Peru and Bolivia, created a dependency on nitrate revenues, warped the economy, encouraged imports and discouraged domestic industrialization, increased the gap between the rich and poor, and made tax reform unnecessary.

Additionally, Sater states that the revolution of 1891 was financed by nitrate revenues and that the parliamentary republic that followed was brought about by bribery and vote buying, also financed by nitrates. That source of funds enabled politicians to insulate themselves from the public's rage. Nitrate revenues declined after World War I with the expansion of the process for synthesizing ammonia. With no funds for patronage the political system collapsed, plunging the nation into a crisis for over a decade.

The author could make a further contribution with a complementary volume focusing on these long-term consequences.

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NATHANIEL DAVIS. *The Last Two Years of Salvador Allende.* Ithaca: Cornell University Press. 1985. Pp. xv, 480. $24.95.

Nathaniel Davis, U.S. ambassador to Chile during 1971–73, recounts one of the most controversial episodes in Latin American history. He tries to refute his demonization as the official who allegedly helped bring about the coup d'etat that resulted in the murder of President Salvador Allende, thou-

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sands of his Popular Unity (UP) supporters, and young American Charles Horman, as depicted in the prize-winning film *Missing*. Davis also hopes to illuminate both the causes of Allende's overthrow and the dilemmas it posed for U.S. foreign policy. As a monograph, however, this book offers little new material or sophisticated analysis. It is also disappointing as a memoir, for it contains few revelations. Nevertheless, specialists will find this blow-by-blow chronicle of events valuable.

The chatty narrative covers the main issues and conflicts leading up to the Blitzkrieg against the democratically elected socialist government in 1973. Although somewhat bland and plodding in the early chapters, the writing gains power and momentum in approaching the showdown. The strongest, most riveting sections treat the military's protracted evolution to insurrection and the day of the assault itself. Davis follows that story with extensive—and predictable—arguments that the blame for that disaster rests primarily on the vacillating Allende and his wild-eyed followers, that his death resulted from suicide rather than assassination, and that the U.S. government was guilty of virtually nothing. Despite some marginal meddling, Washington supposedly pursued no concerted campaign of destabilization, as scores of critics have charged.

On many minor points Davis succeeds in his painstaking effort to be judicious and balanced. He demolishes some of the more polemical accounts of these turbulent years. He makes generous concessions to the other side, praising the charm and idealism of Allende. On major issues, however, the author almost invariably endorses a negative interpretation of the UP and a positive evaluation of its adversaries. According to Davis, the Socialists and Communists were plotting to crush the democratic opposition, whose stalwarts had to be saved by covert U.S. support. If the Marxists were really so bent on strangling pluralism, that their victims could have been rescued by the tiny amount of U.S. assistance that Davis acknowledges is surprising. Further, the author justifies that aid on the grounds of shoring up besieged democratic forces—rather than undermining Allende—and then notes its termination as soon as the junta seized power. Davis is much more willing to give Allende's enemies the benefit of the doubt. For example, he emphasizes that the congress did not directly invite a coup when it declared the UP government illegitimate and that Augusto Pinochet hesitated to unleash the troops.

A similar bias sometimes skews the use of evidence. Davis reports unsubstantiated gossip about corruption and sexual hijinks in Allende's government. By contrast, he takes at face value denials of U.S. involvement in the coup from Pinochet as well as the U.S. Navy, Air Force, and Central Intelligence Agency. Although he subjects allegations of U.S. funding for Allende's nemeses to excruciating examination, he passes along claims of Soviet and Cuban subsidies for the UP with little investigation. Davis's contentions may or may not be correct, but most historians will withhold judgment until more abundant documentation can be assessed by a more neutral scholar. Whoever explores these issues in the future, however, will have to confront Davis's detailed defense of most of Allende's domestic and foreign opponents.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

Totalitarian Democracy and After. (International Colloquium in Memory of Jacob L. Talmon, 1982.) Jerusalem: Israel Academy of Sciences and Humanities or Magnes Press of the Hebrew University. 1984. Pp. viii, 412.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editor's discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

ARTICLES

TO THE EDITOR:

Carl Degler, "In Pursuit of an American History" (*AHR*, 92 [1987]: 5), has referred to me critically in these pages, and I beg space for a reply.

The point of disagreement, to refresh readers' memories, concerns whether national differences are profound or superficial in today's world and in the recent past. To Professor Degler, their profundity seems obvious, and nations remain the fundamental building blocks of history. The point of studying history is to elucidate these national differences through comparisons. In contrast, in an article he quotes, I had argued that "the merger of America into a common pattern of modern life has been the great underlying tendency."

What seems hard for Degler and for many others to grasp is that nationalism and intensely articulated national traditions, while undeniably of fundamental importance in the recent world, at least in political terms, actually comprise one of the most notable *similarities* between all modern states, the only important difference being whether a given state adds to this a desire to play a "great power" role. Belief in one's own national distinctiveness is nearly universal. Claims to exceptionalism, far from being uniquely American, are a dime a dozen—in the form of French pretensions to embody "civilization," one-time German efforts to spread *Kultur*, or any of the other numerous versions that assert some kind of national essence. National assertions are the *common* cause of wars and terrorist movements. They comprise an urgent item of study but one to be glimpsed with detachment (and concern) from the outside, a possible future cause of destruction of the planet. Degler instead would seem to honor these claims—at least those regarding the United States—at face value.

Degler's view of these matters is deeply embodied in the way historical research is now organized and in the manner in which history is taught and written. The automatic demand to present to students a version of "American history," "British history," or "French history" maintains a momentum of its own and leads to the kind of attitude Degler represents. A contrasting way of studying history recognizes the fundamental similarity of all claims to national distinctiveness.

Suppose we were establishing a department of history, including graduate research, from scratch. Would it not make better sense to set up a series of courses for students representing the fundamental realms or compartments of human life—courses in the history of war, diplomacy and statecraft, politics, work, the family, intellectual life, religion, workers, farmers, slavery, cities, ethnic minorities, the middle class, and so on? These courses would draw on local materials taken from any (and at a minimum, several) suggestive national cases. Such realms of human life would be the basic building blocks of the discipline rather than national territories. Future historians would become specialists in the history of one or two of these thematic areas of concern, not in American, British, or French history. They would of course explore genuine national differences turned up by such probing but as much more incidental points than in Degler's formulation. (Differences between early and modern times, and between comparable regions or social settings, would prove more important than national differences, especially in dealing with nonpolitical topics.) Politics and war, the traditional staples of history, would still be studied and subjected to close scrutiny but not from the angle of coaching or inspiring potential local participants in them. I submit that a history department so organized would elicit far more interesting

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research and have far more significant things to say than we do now.

One might respond that such a redefinition of history along thematic lines is utopian. Why? Because powerful intellectual and political pressures, illustrated unfortunately in the thinking of Degler, insist that we present history along national lines, as we now do. Imagine a state legislature, or the board of trustees of a private university, learning that its local university has abandoned the teaching of American or European history as such! Moreover, graduate students are not being trained to offer courses of the thematic kind I have proposed; the language problems seem major to begin with and call for an altogether higher type of student. My point is that the discipline of history is tied by very powerful links with the popular nationalism of the day. Professional history is not nearly as detached as would be helpful in the present effort to stand aside from and control the evil forces at work in the world. And we debate the very idea of objectivity or detachment, rather than moving to implement it. This is because to be truly detached (along these thematic lines) would be unthinkable in terms of the public pressures working on us and channeling our own minds.

To a considerable degree, we historians are part of the problem of nationalism in the modern world by the very way we structure our discipline. We participate, far more deeply than we like to believe, in popular currents of patriotic fervor, whether American or third world. It is sad to see Degler appearing to advocate more of the same.

LAURENCE VEYSEY
Lahaina, Maui, Hawaii

PROFESSOR DEGLER REPLIES:

I welcome Laurence Veysey's disagreement with some of the remarks in my presidential address, since it provides an opportunity to identify the extent to which we have different views of history.

At the beginning, I must make clear that I did not say, or imply, that "the point of studying history is to elucidate" national differences. It is no more than *a* point; it is only one of several purposes of history. It is a purpose because we, as Americans (like other peoples), obtain our identity from our past and, whether one approves of nationalism or not, one has a legitimate reason for a critical inquiry into the sources of one's national identity. That was the purpose underlying my exhortation that we seek to understand ourselves as Americans through comparison with other nations' experiences.

It is true, as Professor Veysey says, that governments require us to teach national history, but that is not the only reason for doing so. Most ordinary Americans, like the ordinary people of other nations, also want us to do so. Like Veysey, my interest in history is instrumental: I believe it is useful. There are more uses of the past, however, than one. I have no theoretical or practical objection to the kind of transnational historical study he describes in his letter. On the contrary, I say more power to him and those who agree with him. It would certainly be another use of history and perhaps a most valuable one if it resulted in a reduction in conflict and misunderstanding among nations. I do object, however, to confining the use of history, as Veysey implies he does, to the achievement of a single political purpose, namely, a reduction in national differences or identities. There are many uses for history: some that reassure us, some that our society requires, and some that may help us achieve a better society in the future. In short, I say, let a hundred histories be written.

CARL N. DEGLER
Stanford University

BOOK REVIEWS

TO THE EDITOR:

Kenneth W. Stein reviews my book, *Zion before Zionism, 1838–1880* (*AHR*, 92 [1987]: 464), damns it with faint praise, and criticizes me for adhering to the limits that my title and preface ought to have warned a reader to expect. I stated quite clearly that the purpose of my book was to describe the forty-two-year period during which European influences developed in Palestine, prior to the advent of political Zionism and the birth of modern Arab nationalism. Professor Stein chooses to criticize me for failing to connect "the period under study with the impact of European consular officials on Arab-Jewish relations . . . from the 1880s to the end of World War I." I cannot understand why the reviewer should insist that the book ought to have been an introduction to the Arab-Israeli conflict. I limited myself to describing the changes in the resident Jewish and Arab communities that later provided a foundation for political Zionism and Arab nationalism. Neither existed in 1880 at the end of the period covered.

Stein states that my separate chapters on Palestinian Islam, Judaism, and Christianity "incorrectly suggest that the religious or ethnic affinities of the monotheistic faiths were distinct and unique in the Palestinian context." This criticism is entirely incomprehensible. I devote a great many pages to the role of the Sultan of Turkey as Caliph ul-Islam, to the development of pan-Islamic revivalism, and the role

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of Arabs generally in Moslem Turkey. I certainly give a great deal of attention to the role of Russia, France, and Britain in the support of client Christian denominations in Palestine. I explore in depth the role of European Jewish communities in supporting Palestinian Judaism. In a word, the international connections of Palestinian monotheistic religion is the most important theme of my book.

The reviewer suggests that my fourteen-page bibliography omits too many major works in foreign languages. His only specific suggestion, however, is that I ought to have made a search of the Hebrew language journal, *Hamizrach Hehadash*.

Finally, my principal complaint against Stein is that he is so busy describing what he perceives as a half-empty glass that he neglects to describe the glass that is half full. He makes no reference to my use of previously unused consular and diplomatic correspondence from British, German, Austrian, American, Vatican, and Israeli archives. I do feel that my work deserves at least the small compliment of a reference to the tables in which I list the consuls, church prelates, chief rabbis, and Turkish governors who served in Jerusalem, 1838–1880. A word of praise for the creation of a unique reference tool would have been in order.

A person who has sufficient professional stature to be invited to prepare a review for the *AHR* ought to be sensitive to the fact that a heavy burden is thereby assumed. The *AHR* is the journal of record for our profession in the United States. A reviewer has the primary obligation to describe positive elements in a colleague's work, knowing that years of effort are at stake. To drown a book in a haystack of quibbling complaint is irresponsible.

ARNOLD BLUMBERG
Towson State University

PROFESSOR STEIN REPLIES:

Unfortunately, Professor Arnold Blumberg is reluctant to accept even the mildest criticism of his work, *Zion before Zionism, 1838–1880*. Blumberg was kind enough in his book's acknowledgments to thank me as one of those "distinguished colleagues" to whom he owes gratitude (p. xiv). It was certainly an unnecessary but a warmly appreciated expression of thanks, since I did little more than make some minor suggestions about archival facilities, their availability, and source material. But when fellow historians ask for assistance, our professional responsibility is to be collegial. We are equally responsible for writing a fair and informed review: for assessing substance, sources, and the book's place in the present body of historical literature.

In the absence of a less-than-glowing assessment of his work, Blumberg exaggerates my remarks as a "haystack of quibbling complaint." He categorizes my review as "irresponsible." Indeed, no one doubts that the author labored long and hard in writing this book. But, unlike his complaint, my review was restrained, words were carefully chosen, and volume was precisely measured.

After reading Blumberg's letter, I am not sure he read my review closely. Nowhere did I assert or imply that the author failed to discuss the relationship of the European powers with the indigenous Moslem, Christian, or Jewish communities in Palestine. Blumberg does not prove in three very short but separate chapters that Palestinian Islam, Palestinian Judaism, or Palestinian Christianity were uniquely Palestinian. Blumberg misses my point. Yes, he discussed, in considerable depth, the European relationship with the indigenous populations who practiced the monotheistic religions. No, he did not make a case that Islam in Palestine was different from Islam in Egypt or that Judaism in Palestine was different from Judaism elsewhere in the Middle East at the time. Maybe he did not want to make that point. But his choices for these chapter titles, "Palestinian Islam," "Palestinian Judaism," and "Palestinian Christianity," are misleading and totally inappropriate for their respective contents.

Nowhere in the review did I criticize the author for failure to use *archival* sources; my criticism was of his limited use of *published* source materials. He lists articles in the bibliography that appeared in the Hebrew journal *Cathedra*, but few if any of them are cited in the footnotes. Excellent article-length publications in English, which focus on foreign involvement in mid-nineteenth century Palestine and which were published before Blumberg's book, were not part of the author's bibliography. These include works by Alex Carmel, Mordechai Eliav, Isaiah Friedman, Haim Gerber, J. C. Hurewitz, William M. Kramer, David Kushner, Sergio Minerbi, Norton B. Stern, and Chaim Wardi. Blumberg's bibliography includes neither Moshe Ma'oz's important edited volume, *Studies on Palestine During the Ottoman Period* (Magnes Press, 1975), nor the vast array of German and French monographic sources for the late Ottoman period, especially those published during and after the 1838–1880 period. I stand by my remarks that Blumberg's book is incomplete in its survey, use, and analysis of published sources.

Nowhere in the review did I state or imply that "the book ought to be an introduction to the Arab-Israeli conflict." These are Blumberg's words and characterizations, not mine. It is in fact Blumberg who says in his book's preface that these "forty-two years form the unique and incubative time period without which Zionism could never have prospered in Zion" (p. xi). Blumberg, in his letter, paraphrases or corrects himself by saying that the purpose of his

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book was to study this time period "prior to the advent of political Zionism and the birth of Modern Arab nationalism." He should have clarified this point in his preface, not in a letter to the editor of the *AHR*. But to joust over words is to evade the point: each scholarly work must be put into a proper context for fellow academics, researchers, and students. The author did not place his findings in a context for either the period before 1838 or after 1880. Why were these forty-two years an incubative period?

Finally, Blumberg is a historian of considerable accomplishment. His dedication to his craft is not doubted just because a critical review is written. Neither Moses nor Muhammad had an eleventh commandment that said, "Thou shalt not write a critical book review." If I were a historian with Blumberg's experience, I would have hesitated before using terms such as "incomprehensible" and "irresponsible" unless I were absolutely sure that the remarks I was challenging had no merit at all.

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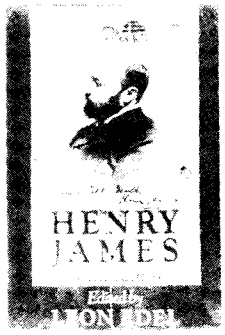
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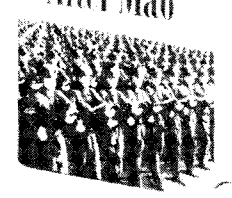
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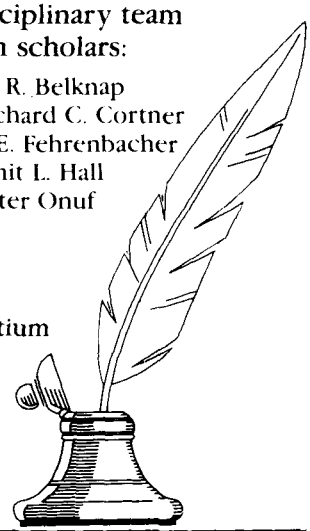
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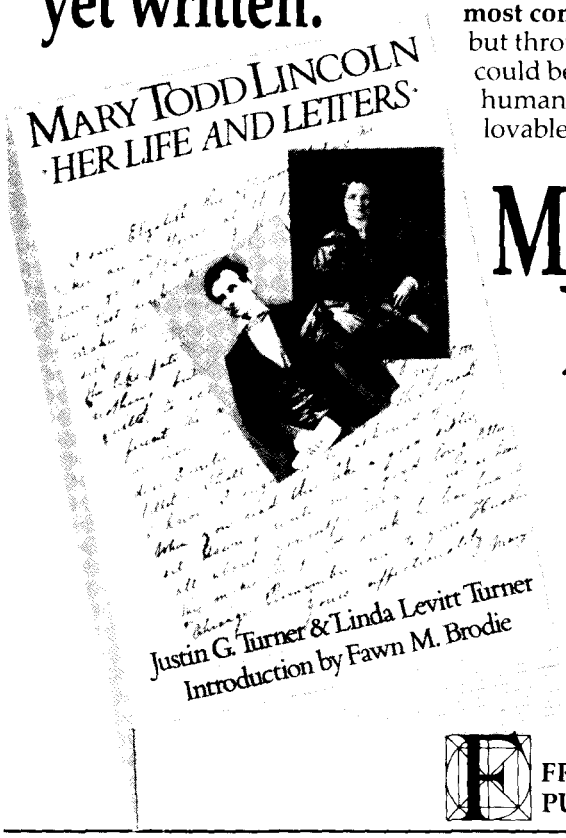
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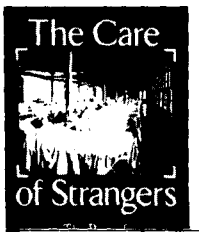
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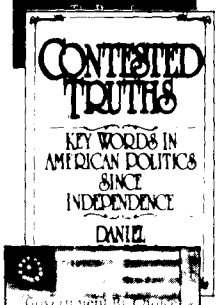


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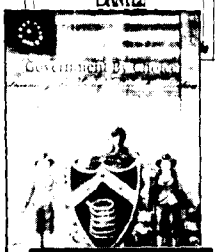


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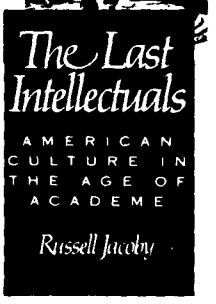


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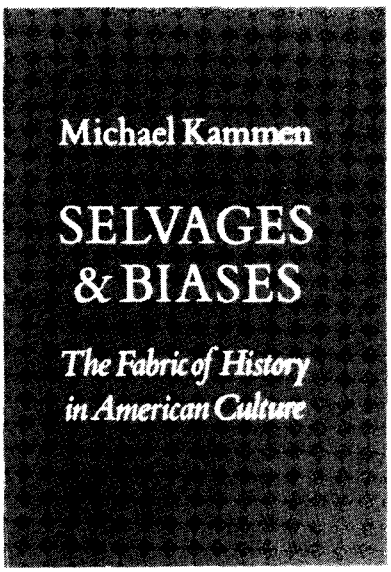
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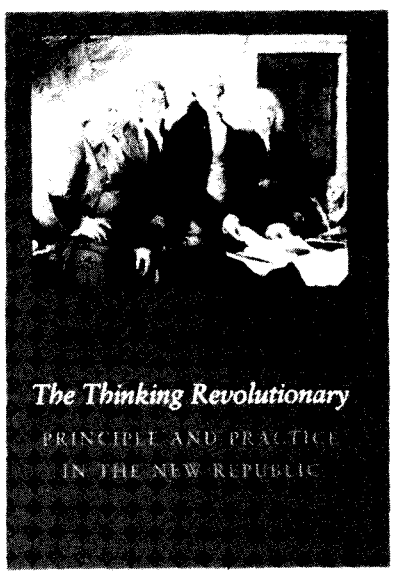
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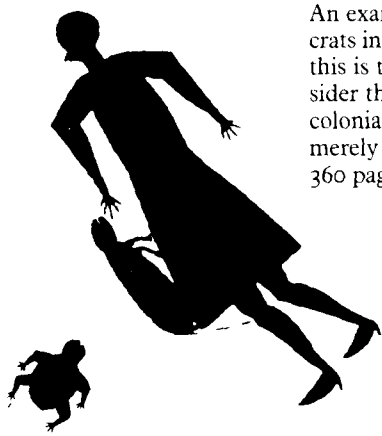
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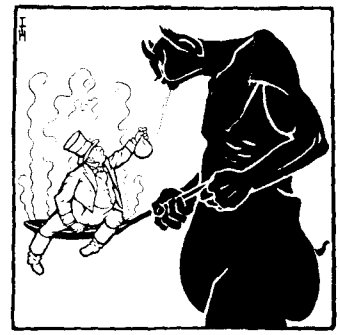
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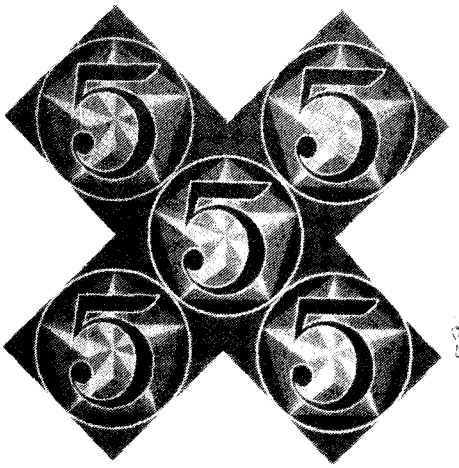
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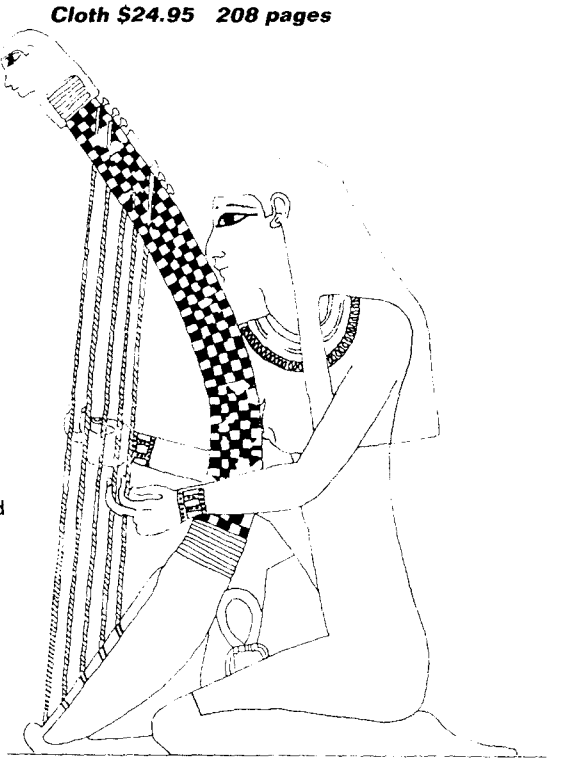
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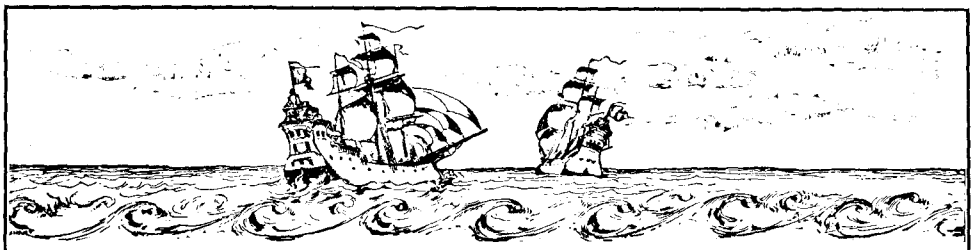


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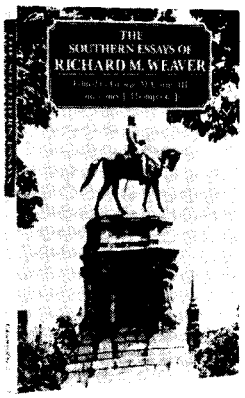
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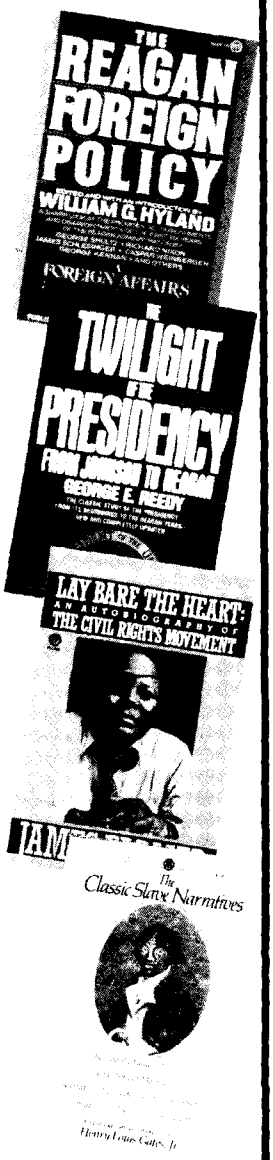
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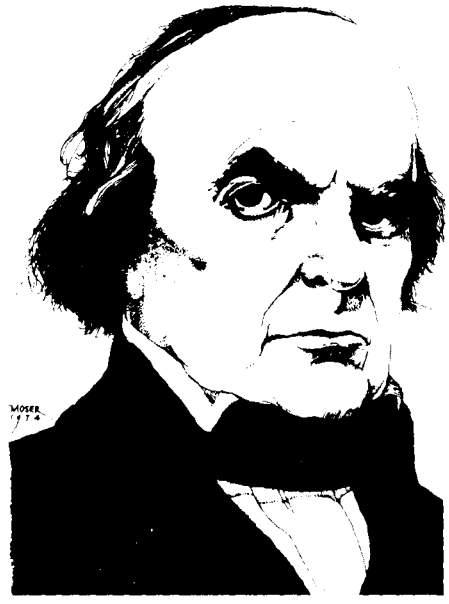
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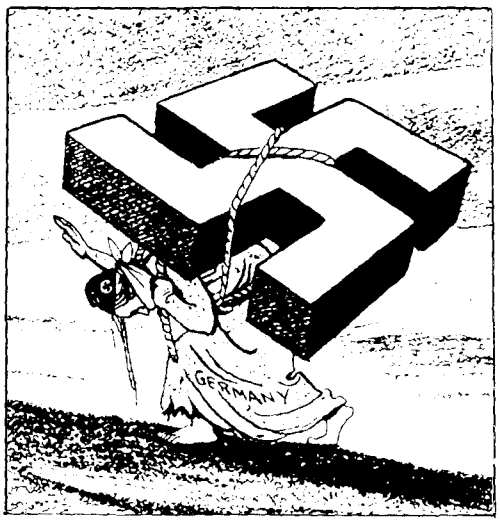
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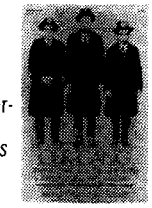
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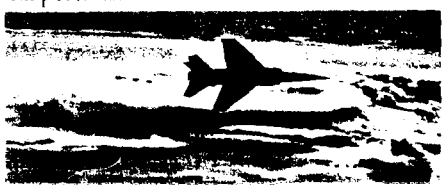
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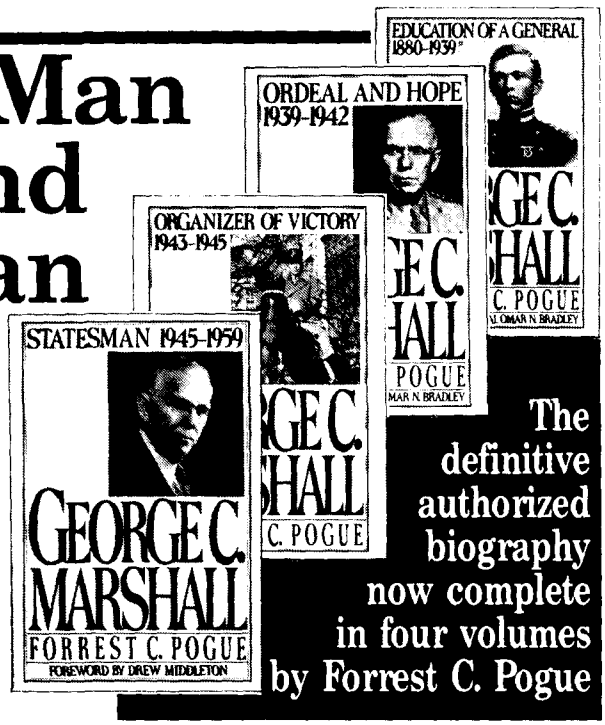
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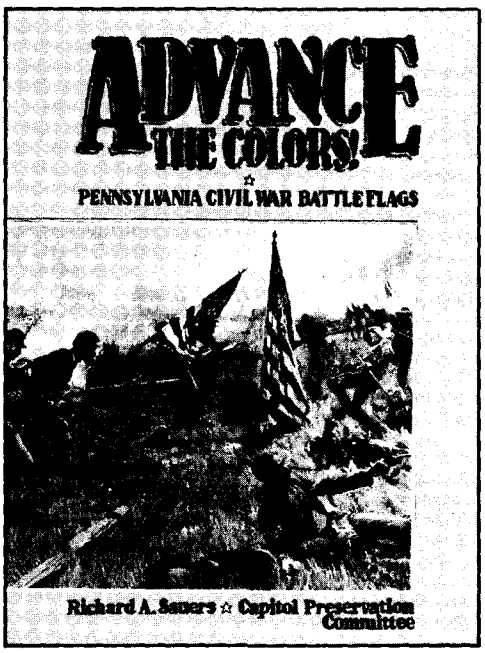
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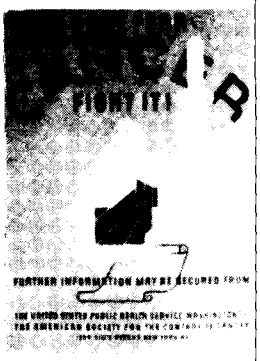
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